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# Science

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(See page 162)

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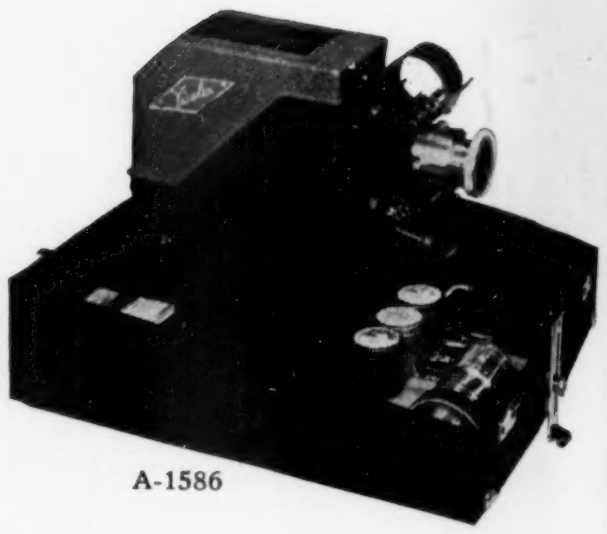

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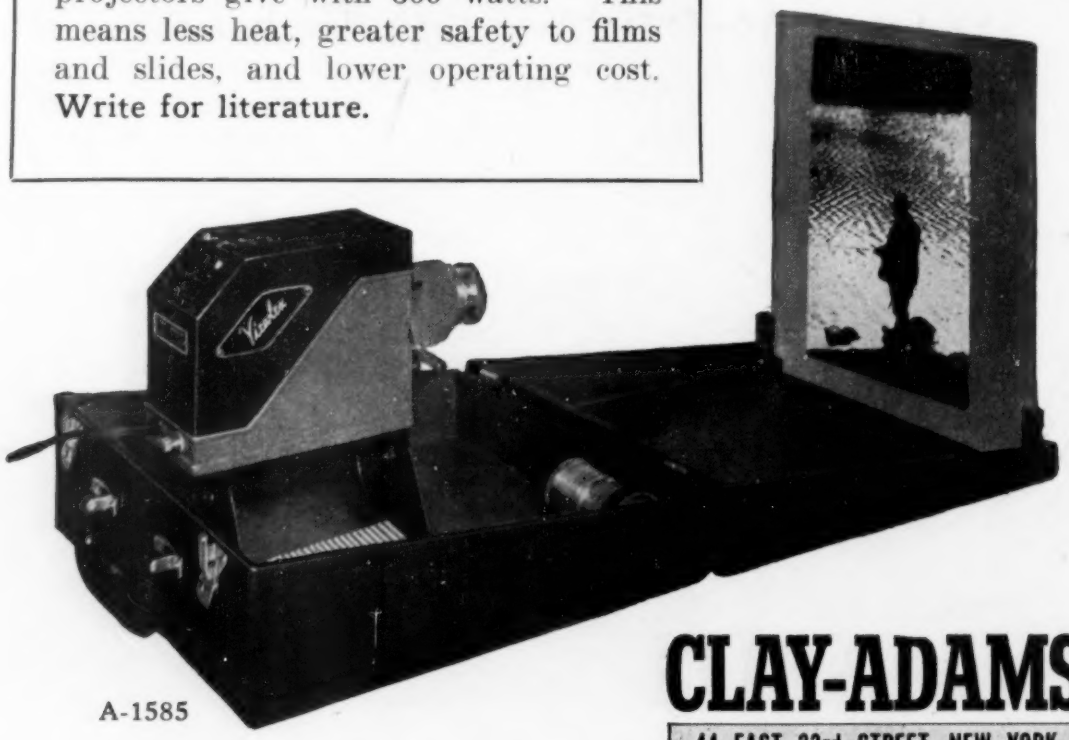

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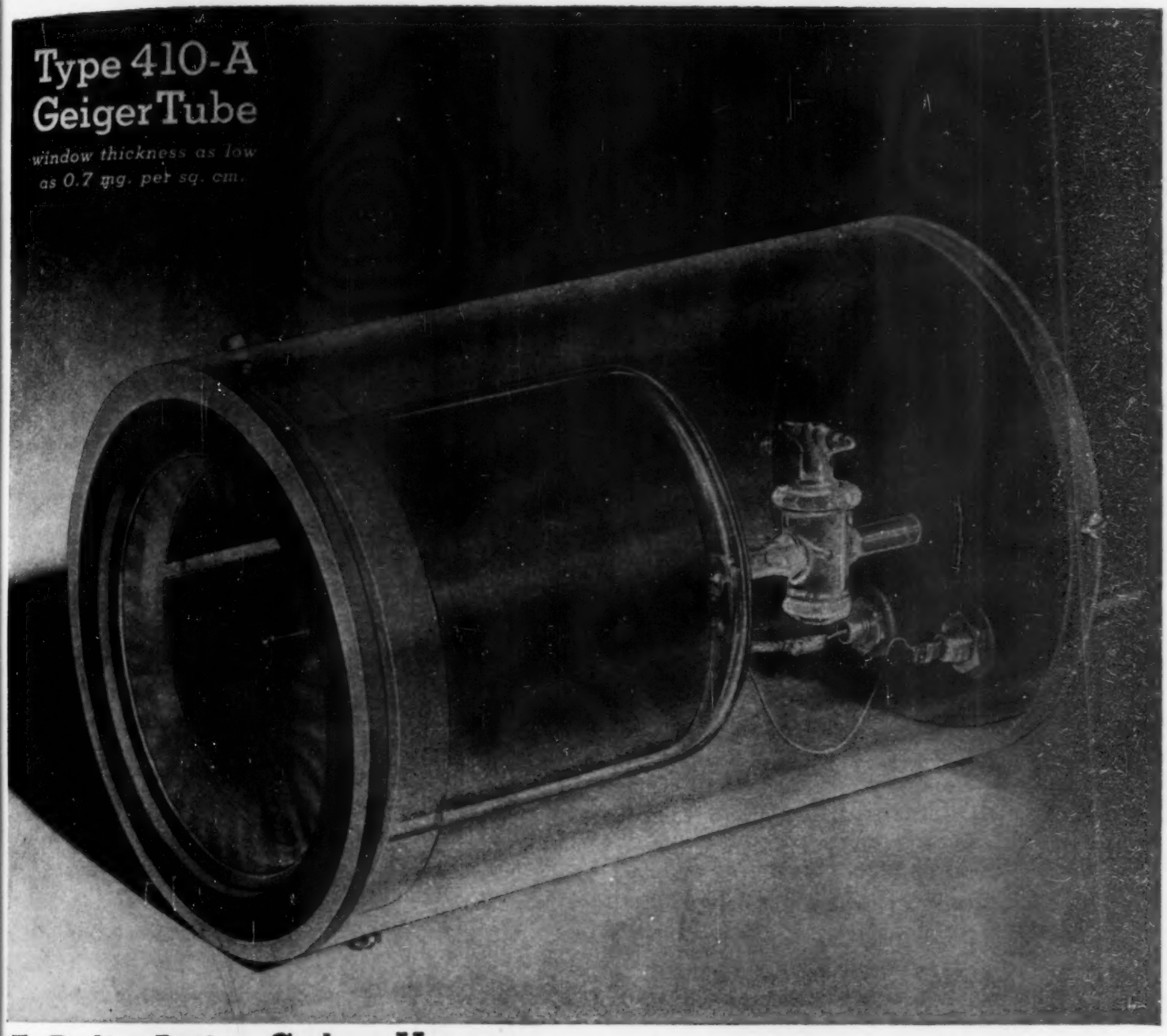


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# Three Quarter-Centuries of Cytology

**Franz Schrader**

*Department of Zoology, Columbia University*

IT IS ALWAYS DIFFICULT TO MAKE a fair comparison of the various periods that mark the development of any field of science. The pioneers who open up a virgin field are often endowed with abilities of a kind different from those characterizing the men who later develop that field. Nevertheless, most scientists will come to think of a certain period as the glorious one, of another as marking time or decline, and so on. Such a differentiation into periods is especially easy in the history of cytology, but, as elsewhere, the evaluation of these periods is definitely a matter of argument.

The history of cytology falls rather naturally into quarter-centuries. Every biologist is familiar with the first of these, which began about 1875 and terminated in 1900. This is the period of breath-taking discoveries and great expectations in which the names of Strasburger, van Beneden, the two Hertwigs, Boveri, and many others are associated with the first real recognition of the mitotic apparatus, fertilization, chromosomes, chondriosomes, gamete formation, and early embryology. It was an exciting period, pregnant with the conviction that we were on the verge of uncovering the secrets of the life processes and of life itself, and it was also a time in which there was some bitter controversy about questions of priority and ethics. There was, in any case, no doubt that cytology represented an independent field of research which was at the very spearpoint of progress of biological advance.

Though the various cytological researches into mitosis, fertilization, parthenogenesis, and embryology were continued triumphantly into the second quarter-century, the feeling that the answers to all our large questions lay almost in our grasp gradually weakened. The new and most striking development in cytology during the years from 1900 to 1925 lay in its alliance with genetics. These two disciplines started out on an almost equal footing as far as the new study of heredity was concerned, for the rediscovery of Mendel's findings at the turn of the century was followed within a year or so by Sutton's and Boveri's cytological demonstrations that Mendel's laws are closely tied to the chromosomes. The acceptance of that relationship was not an immediate one. It was only after the correlation between sex determination and sex chromosomes had been established by McClung, Wilson, and Stevens, after the convincing evidence carried by Bridges' nondisjunction experiments had been comprehended, after Carother's demonstration of random assortment had admitted of no alternative answer, and finally, after the accumulation of genetic discoveries had become really overwhelming, that the general biologist resisted no longer. These years seemed to mark the closest cooperation between cytology and genetics, as has often been stated. But toward the end of this quarter-century it appeared to the insider that the cytologist was not keeping pace with the incredibly rapid advance of his co-worker.

This dropping behind of the cytologist merits a closer examination. It must first be realized that the findings of the geneticist were usually capable of standing alone on their mathematical basis. He recognized, however, that cytology might often give him a lead to new discoveries and that his arguments could be made more convincing to biologists in general if he could say: "There can be no doubt about the correctness of my conclusions. You can actually see the mechanism at work under the microscope." Moreover, there were cases where the genetic analysis alone gave no final answer, and then the appeal to cytology was in the nature of a request for aid. Willy-nilly, the cytologist thus came to play the secondary role of the supporter, and in this role he was often expected to furnish the capstone that made the proof unassailable (again witness Bridges' cytological demonstration of nondisjunction). His prime virtue thus lay in utter reliability, and the responsibility for this began to weigh heavily upon him; he became extremely cautious, he guarded and double guarded each of his forward steps, and in his hesitation finally to commit himself he became the archdisciple of Ruskin's characterization: "In science you must not talk before you know."

It was part of this attitude of caution that also made the cytologist accept wholeheartedly Bateson's advice to "treasure your exceptions," for to him all exceptional and aberrant cases were natural experiments that constituted tests of the generalizations that he was making. When the cytologist could not get at the bottom of one of these exceptional cases, and when no soundly based explanation was available, he care-

Address by the vice-president and chairman of the Section of Zoological Sciences (F), AAAS, delivered at the Zoologists' Dinner, December 30, 1947, in Chicago, Illinois.

fully described it and filed it away in one of the scientific periodicals—somewhat in the way that valuable but useless articles are stored in the attic. Finally, the spirit of criticism was very strong within him, and any new finding or idea by one of his co-workers was immediately subjected to the most painstaking examination. It all tended to hinder progress, though, as a matter of fact, the groundwork for much future advance was being laid at this time. But it must be admitted that to the geneticist much of it was of no immediate use. Also, the cytologist gradually lost much of his value as a path-breaker and often seemed to be in the position of the doubting Thomas who did little to help the cause along.

An illustration of this position is shown in the matter of crossing over. The geneticist's evidence that some kind of interchange may occur between homologous chromosomes was quite clear, and Janssens had shown that chiasmata might well represent direct visual evidence for such a process. Rightly enough, the cytologists did not consider Janssens' evidence as conclusive, but they could not bring forward anything better on this highly important question. It was not until 20-odd years later that Stern, and Creighton and McClintock, furnished the much-needed proof. During all these years the cytologist sat rather uncomfortably and merely shook his head.

The close of this second quarter-century in 1925 was signalized by the publication of Wilson's great work on the cell. This book shows very well the conditions then obtaining in cytology. There had been many great advances, but there also had accumulated a host of pertinent questions which had no answers, as well as a great number of findings which seemed to have no application anywhere. The volume of all this was truly immense and had assumed such proportions that it has often been said that Wilson was perhaps the last man really to encompass all of it.

The beginning of the third quarter-century is marked almost as sharply as is the end of the second. It was Belling who ushered in this, our present, period, and his real claim to fame was his recognition of the experimental value of extra, or supernumerary, chromosomes in the analysis of meiotic phenomena. His skillful elucidation of chromosomal behavior (for which Blakeslee was furnishing the genetic background) opened up avenues, the vista of which no one before had fully appreciated. To be sure, both Wilson and Bridges had dealt with heteroploid conditions, but not with any intention of analyzing chromosome mechanics as such. The unique advantage of heteroploidy to the experimentalist lies in the fact that it usually does not involve detrimental effects on the life of the cell such as nearly always are the concomitants of surgical intrusions or physicochemical treatments; as Bleier has said, "probably there is no more harmless and elegant a method of intrusion into the nucleus." In Belling's recognition of the experimental value of heteroploidy and polyploidy lay the germ of most of the advances that have been made during recent years.

However, not many cytologists and very few geneticists recognized the significance of Belling's contributions at once. In his work Belling was adhering strictly to the traditional principles of cytology, which meant that his advances were made cautiously and therefore slowly. There was no immediate prospect that he would attack the growing mass of cytological data to make it available to geneticists or noncytologists in general. Given time, he might have done so, but his death in 1933 terminated his activities much too soon.

In 1929, however, two joint papers on heteroploidy in certain plants were published by Newton and Darlington. These heralded the advent of a new departure in cytology—a departure which was exactly what most geneticists had been waiting for. Since Newton died before these papers were published, it was Darlington who was mainly responsible for the generalizations and who went on to develop the "New Cytology." This had its first general expression in 1932 in his *Recent advances in cytology*.

Not often has a single man made such an impression on old established fields of science as has Darlington on cytology and genetics. Under the impact of his attack the unwieldy and undigested mass of cytological data seemed to dissolve and reprecipitate as a usable system. Viewing this change, the geneticist gained a new sense of power and once more began to look on cytology as an aid to his researches and something that he could himself employ. His change of attitude was reflected in various ways. Nearly all of the younger geneticists began to add cytological methods to those which were traditionally genetic, and even the older workers frequently did so. In short, these geneticists became cytogeneticists. Generally, to be sure, the make-up of such a cytogeneticist was still largely genetical—say, to the extent of 80 or 90%—but a portion of him was definitely cytological, and he tackled his problems with a buoyancy, a fresh courage, that had been lacking for some years. The change is shown in various ways. At the Christmas meetings of the American Genetics Society in 1929, before this revolution, 5 out of the 39 papers presented (*i.e.* 13%) involved some cytological work on the part of the author. By 1940, 11 years later, the papers which employed cytological methods in part had increased to 52% of the total. In other words, during this significant decade the percentage had quadrupled, and it is safe to say that many among

the remaining papers employed some cytological reasoning even when they did not actually use a compound microscope in solving their problems. How did Darlington—for certainly his was the predominant influence in this change—bring this about?

To comprehend this, it must be realized that the change involved not merely the fashioning of new and ingenious tools of reasoning, but also rather fundamental changes of working principle and viewpoint. When stated on paper, these changes do not seem so exciting, but it must be remembered that they affected the fundamentals of a field that had a great tradition and much historic weight. Darlington, who with rare penetration had made an analysis of the needs of the geneticist and the difficulties of the cytologist, said in effect: "Let us cut the Gordian knot and reduce this mass of cytological information to what is essential, always keeping in mind the basic conclusions that have been reached through genetic procedure. We can ignore the remainder for the present, for the seeming contradictions will fall into line as time goes on. And, contrary to Ruskin, let us not be afraid of making our hypotheses public." Through this process of winnowing out the worth-while material, Darlington arrived at a very few basic generalizations, simple almost to the point of naiveté, but of immediate utility in nearly all genetic considerations.

The first and most fundamental of these generalizations is simply that, at a certain point in meiotic prophase, homologous chromosome regions attract each other in pairs, and only in pairs. If another, or third, homologous region is present in the nucleus, it will not be attracted to the region where such pairing has already occurred; in fact, it will even be repelled. But this rule applies not only to whole chromosomes but also to the longitudinal subdivisions of chromosomes. Thus, if a chromosome is already split (*i.e.* composed of two chromatids), the affinity for an association in pairs is thereby satisfied, and no other homologue is attracted at all.

It may be said that this was not new and that, for instance, Wilson and Morgan had carefully considered the rules of synapsis many years before. But they had not considered the various possibilities of numerical variations, and what would happen if a third homologue is present had not really entered their considerations. In this lay a most important key to the explanation of chromosome behavior.

The second rule is almost but not quite as basic. According to Darlington, after the homologous, single chromosomes have joined in pairs during meiosis, each one splits. At about this time there may occur an interchange between two of the four resulting chromatids. The tendency of the four chromatids is to repel one pair from the other, but if the interchange has taken place between chromatids of two different chromosomes, a bridge or chiasma will prevent this separation. A chiasma is therefore visual evidence of one of the most important processes in evolutionary and meiotic mechanics, and the presence of crosses, rings, and similar configurations involving four chromatids is sufficient evidence that this important process—that is, crossing over—has occurred.

Darlington's elaboration of these two basic rules and their employment in the solution of many cytological questions of long standing were at all times resourceful and occasionally brilliant. Thus, the factors that bring about meiosis, the mechanics of mitosis, the structure and function of the kinetochore, and many more problems were explained on the basis of these fundamental generalizations. Indeed, no question came up that did not receive an explanation of some sort, and these answers were given with superb confidence and authority.

The geneticists accepted the help given them by the "new cytology" with appropriate thankfulness. This is reflected very nicely in the textbooks that have been written since 1932. Without exception, all of these books utilizing cytogenetics regard Darlington's rules as a constituent part of the science, and it is very rare indeed that even a hesitation is expressed over accepting his conclusions. This represents very fairly the attitude of the cytogeneticists and geneticists in general, and there can be no doubt, as I have already said, that the effect was an inspiring one.

However, that is not to say that there were not some exceptions among the cytogeneticists who did not use these tempting tools quite so uncritically and who retained a certain concern over the underlying verities; after all, there is a McClintock. There was also still a group of cytologists who, true to their basic and, if you wish, old-fashioned principles, did not accept these generalizations of the "new cytology" so wholeheartedly. As might be expected, they objected, first, that in selecting the data that supported each generalization, much pertinent information had been ignored because it was contrary to the hypothesis arrived at. The very last publication of Belling, in 1933, has some 25 pages of objections to various statements and conclusions reached in Darlington's book, and the reviews written by some other cytologists expressed similar dissatisfaction even while voicing admiration for the boldness and ingeniousness of the reasoning.

These objections have not diminished in the years since 1932, at least as far as the cytologists are concerned. Whenever they are specific, they can certainly not be ignored, and I will take time to outline two of them because they affect the very basis of the whole structure:

(1) I have already emphasized the most basic of these rules, the one that in some form or fashion underlies all of the others, to wit: it is the split and unsplit conditions of the chromosome that determine its behavior with respect to homologous chromosomes also present in the nucleus. Although such a generalization does not explain what the underlying forces may be, merely to establish the existence of such a numerical rule of behavior is immensely important. Aside from its immediate applicability to genetic reasoning, it would seem to point out the direction which the final attack on this all-important question in cellular mechanics must take.

The trouble is that almost all cytologists, except for Darlington and his followers, are now convinced that the factual findings are erroneous. Each chromosome is already split in telophase—that is, it is then composed of two chromatids, and the evidence is mounting that there is even a subdivision into half chromatids. In short, unless there is then some mysterious healing just at the time of synapsis, we are dealing with two or four parted chromosomes instead of single, unsplit ones. It has been suggested that the situation can be saved by considering such chromosomes to be "physiologically single." Although this may be a legitimate restatement of the actual conditions, it cannot, however, hide the fact that the numerical generalization which seemed to open the way to the final solution has irretrievably been lost. It is not the singleness of a chromosome which underlies its attraction to another single homologue, and, as Huskins states it, "the general problem now appears to be not to what degree the chromosome is subdivided, but how it comes to behave as a bipartite unit in inheritance."

(2) The second generalization, only less important than the first, that the chiasma represents an interchange between two chromosomes and that it serves to hold such chromosomes together even when they are split, still is valid as far as it goes. But, to the cytologist, it is a matter of importance that there are other forces as well which must be considered in the formation of tetrads, ranging from terminal and somatic attraction to heterochromatin, collochores, and other, yet unexplained, mechanisms. Indeed, in some cases such alternative mechanisms seem to have entirely replaced the chiasma, which can therefore no longer be considered the sine qua non in tetrad formation. Darlington himself now recognizes the existence of several of these additional factors, though his co-workers have not always done so, and the identification of chiasmata by cytogeneticists in general has not always been above cavil.

These criticisms of the two fundamental generalizations, of course, also affect the more subsidiary ones. Suffice it to say here that much of the foundation on which modern cytogenetics has been erected is in need of reconstruction—although this may be possible without destroying all of the superstructure that the cytogeneticist has erected on it. It should here be pointed out that the cytologist's task of examining this foundation was at times a thankless one, for he played the role of the man who pokes sticks into the wheels of a nicely rolling vehicle. Furthermore, he was free to admit that his own accumulation of unexplained facts had begun to hinder progress, and that the formulation of generalizations at least set up targets at which to shoot. His complaint was that the cytogeneticists as a group never shot at them, and possibly they can not be blamed too much for that. They were busy enough as it was, and one can do just so much in one's working day. However, it seems not too much to ask that they recognize the necessity for such a testing of the rules that have been set up, and that the cytologist for whom the analysis of cell structure and function constitutes the primary interest is duty bound to make such tests. I fear that the cytogeneticist has not always recognized that, when the cytologist is remiss in this respect, both disciplines will suffer in the end, by either a slowing of progress or waste of effort. Let me illustrate:

The salivary chromosomes represent somatically paired homologues of huge size, which are strikingly marked by transverse bands, individually recognizable. These bands are correlated in position with the loci of genes. The recognition of these general facts by Painter, Heitz, and Bauer has given us one of the most useful tools imaginable, and its employment by the student of evolution has opened up paths that have resulted in really great advances in that old field of endeavor. But, oddly enough, today—14 years after the great utility of the salivary chromosomes was first recognized—we still do not know their exact nature, and we are still puzzled about the origin and structure of the transverse bands. There is no denying that the answers to these problems are not there for the mere picking up; nevertheless, it is remarkable that scores, or even hundreds, of workers have used the salivary chromosomes as a tool in their work, whereas a scant half dozen or so have seriously concerned themselves with their more basic aspects. Our ignorance is now beginning to prove a hindrance, and the question arises of why more work has not been done to remove it. I think the answer lies in the fact that the question involved is primarily cytological and that the ranks of the cytologists have become so thinned out that there are not enough to do all the work.

Here is a second illustration: The male of *Drosophila* has no crossing over, and in conformity to the correlation between crossing over and chiasmata, the

autosomal bivalents show no chiasmata. However, the x and y chromosomes enter into a combination that results in a normal, cross-shaped tetrad. Since on the Darlington hypothesis this must mean that a chiasma is involved, why is there no genetic evidence of crossing over? Darlington's explanation is characteristically ingenious. Crossing over does indeed occur in the sex chromosomes, but it is always restricted to a single region which is devoid of active genes. Further, since a single cross over would interchange an arm of the x with an arm of the y—which certainly does not occur—he postulated that there is always a reciprocal cross over, which means that in the immediate vicinity of the first there is another one which brings both arms of the x (and y) together again. An apparent exception to his basic rule is thus shown to constitute a striking confirmation. But this explanation, as Cooper has recently shown, involves three major assumptions as well as eight steps in the cytological argument which admit of alternative explanations. Undeniably, the hypothesis is clever, but the cytologist of 25 years ago would never have accepted it as it stands. The "new cytology" presents a striking contrast to that older attitude, for in the 10 years prior to Cooper's analysis no one seriously questioned Darlington's explanation, and at least half a dozen cytogeneticists undertook extensive investigations which were based on a full acceptance of all of its points. If the moral is not obvious, it must at least be recognized that the waste of effort that results from such a state of affairs may well be very considerable.

I have said enough to portray the status of cytology during the years since 1925. It is a period dominated by bold generalizations which have induced a vast amount of research with some splendid results, chiefly by the cytogeneticists. But work on the fundamental questions to which we must all return sooner or later has not progressed so satisfactorily, and the relatively small group of cytologists that have kept it in mind have not been rewarded with much appreciation. As I have already noted, that is largely due to the mild contempt with which other workers have come to regard the painstaking principles of the cytology of former years. But, in view of the weaknesses that I have pointed out, I would venture the opinion that a judicious return to some of those principles would exert a decidedly wholesome influence in the present day. If the free and untrammeled making of speculations and hypotheses continues, these will gradually encumber our movements as much as did the piling up of unsolved cytological puzzles in former years. Destructive criticism, if constructive cannot be had, had better not be removed from the cytological arsenal; the restless questioning of a Belling and the coldly analytical mind of a Belar have been sorely missed during the past 15 years. Nor can we afford to hold our noses quite so high as we encounter the aberrant cases that trouble our beautiful generalizations; after all, in the cytology of *Drosophila* itself there is much that does not conform to what we have set up as the standard course of events.

It is taking no great risk to predict that 1950 will mark the end of a period, just as sharply as did 1925 and 1900. Whether he is pleased or not, the cytologist of the next quarter-century will find his co-workers in the laboratories of the biochemist and biophysicist—if the handwriting that workers like Caspersson and Mirsky have put on the wall means anything at all. Moreover, it is already obvious that in this newer alliance the cytologist will not play the subsidiary role that he has had in the recent past, for it is on the foundations constructed by him that this building of the future will arise.

# *Association Affairs*

## Centennial Meeting Plans

*Membership drive*: Commencing on January 6, invitations have been sent from the office of the administrative secretary to a list of nominees submitted to this office by associate members of the Centennial Membership Committee. As of January 29, over 1,000 of the 16,000 scientists invited to join the AAAS had become new members. Unfortunately, a large number of invitations to members of educational institutions could not be mailed for lack of a specific departmental address on the nomination forms.

*Meeting plans*: The Centennial Policy Committee, at a meeting on January 24–25, drew up a general outline of the September meeting arrangements. As presently planned, the morning sessions on September 14, 15, and 16 will consist of 5 concurrent technical symposia to commence at 9:30 A.M. and close not later than 12:30 P.M. The mid-day program, as announced previously (*Science*, December 5, 1947, p. 535), will be free of meetings to permit a series of afternoon activities to be determined by the local committee. The evening sessions will include three concurrent symposia of a semipopular nature which will commence at 8:30 P.M.

The topics for the morning sessions have been chosen deliberately in order that the subjects may be integrated about a central theme of interest to various scientific disciplines. The three speakers on each symposium will be limited to 30 minutes for presentation of their papers. In addition to the speakers, one to three discussants will be assigned to each session, each to follow a designated speaker. A maximum of 10 minutes will be devoted to these discussions. In order to permit audience participation, those in attendance may submit written questions for the speakers to answer toward the close of the session. If enough interest is evidenced by the questions, the sessions may run beyond 12:30 P.M., but not beyond 1:00, in order to permit general participation in the afternoon activities.

Titles of the morning symposia tentatively proposed were: "Problems of the Ocean," "World's Natural Resources," "The Upper Atmosphere," "Human Individuality," "Sources of Energy," "Food," "World Health Problems," "Early Man," "Genes and Cytoplasm," "Science Education and Interpretation," "Gerontology," "Waves and Their Uses," "Nucleonics," "Solid States," and "High Polymers." Some of the suggestions for evening lectures were: "Populations," "Weather Control," "Medical Research," "Human Frontiers," "The World of Isotopes," "Giant Machines for Research," "Bird Migration," "Conservation of Soil," "Wood and Its Uses," "Mechanical Brains," "Housing," and "One World of Physics."

It is the consensus of the Policy Committee that the Centennial Meeting should include as speakers individuals who are most actively engaged in expanding the horizons of science.

## Section on Astronomy (D)

The Section on Astronomy met on the afternoon of Friday, December 26, on the forenoon of Saturday, December 27, and jointly with Section E (Geology) on the afternoon of Saturday, December 27. The joint meeting was a symposium on "Origin of the Earth."

The papers presented may be grouped as follows: address of the retiring vice-president, miscellaneous papers, meteors and meteorites, and on the origin of the earth. They will be summarized in that order, rather than in order of presentation.

*Address of the retiring vice-president.* G. Van Biesbroeck reported that his work on the eclipse of May 20, 1947, gave for the Einstein deflection 2".00, a value some 15% larger than the calculated. This is in good agreement with earlier observations, but all observed results are somewhat uncertain.

*Miscellaneous papers.* C. H. Smiley reported that photometric measures of light reflected from clouds had given, for two eclipses, good results for times of contacts. Astronomers can obtain these times even if a cloud covers the sun.

C. C. Wylie commented on the hysteria produced by unexplained reports of strange objects in the sky. Meteor workers have usually been able to obtain, at little expense, the facts back of these reports. A nation-wide and year-round "Patrol," organized by meteor workers to investigate such reports, was therefore recommended.

It was proposed by D. H. Menzel and W. W. Salisbury that cosmic rays are produced by solar radiation of very long wave length (200–2,000,000 miles), which accelerates interplanetary ions.

O. C. Collins recommended that a navigator obtain a "fix" by observing two stars at the same altitude, or at the same azimuth.

That the calcium lines in RS Canum Venaticorum appear to come from huge prominences restricted to a small area on the large star was reported in a paper presented by W. A. Hiltner.

W. P. Bidelman reported that the spectrum of Rho Cassiopeiae has changed remarkably and is now similar to that of a very luminous K-type star.

G. Kuiper's paper dealt with infrared observations of the planets and, in particular, the finding of carbon dioxide in the atmosphere of Mars.

*Meteors and meteorites.* P. M. Millman discussed observations of the spectra of the Giacobinid meteors. These spectra show important differences from those of the Perseids, which can be attributed to the lower velocity of the Giacobinids.

C. C. Wylie reported that the orbits of three meteors from which meteorites have been recovered are of the asteroid type. These, and similar orbits for other detonating meteors, suggest that meteorites are fragments from the hypothetical planet whose disruption produced the asteroids.

Carl A. Bauer stated that the extreme differences found for the age of meteorites had caused astronomers to doubt their common origin in this hypothetical planet. But, he pointed out, cosmic radiation produces "extra" helium, especially in the smaller meteorites, and this has led scientists to believe them older than they really are.

Harrison S. Brown showed that, if the elements in meteorites represent equilibrium distributions, the meteorites presumably had their origin in a planet similar to the earth, or to Mars. Dr. Brown constructed a model of this ancient planet.

*On the origin of the earth.* In this symposium, Kirtley F. Mather stated that presumably the astronomer should have the task of developing a theory to account for the origin of the earth, but the geologist should give the specifications for a newborn earth that would evolve into the earth as it is today. He then sketched the stratiform structure of the earth's interior. James B. Macelwane described the seismological methods of arriving at the structure of the earth's interior, giving the results rather fully. Harrison S. Brown, in the paper previously mentioned, discussed the structure of the earth's interior and, by correcting for pressure differences, showed that Mars must be strikingly similar. W. J. Luyten's paper (read by title in his absence) outlined the Nebular Hypothesis of Laplace, the Planetesimal Hypothesis of Chamberlin and Moulton, and, more briefly, certain more modern theories. There are seemingly fatal objections to all of these. J. Allen Hynek's paper (read by D. B. McLaughlin) outlined the Wiezsacker theory, which predicts the spacing of the planets according to Bode's law. D. ter Haar showed that for the formation of the earth from gaseous material, the temperature must have been between 400°K and 1,000°K; but the molten earth might have attained much higher temperatures from energy released during condensation. A newly-developed theory by which the planets developed in a large cloud of interstellar gas and dust, as it collapsed to form the sun, was outlined by Fred L. Whipple. The planets began as condensations in a smaller cloud of the assembly and spiraled inward.

*Presiding officers and attendance.* For the Friday afternoon and Saturday forenoon sessions of Section D, D. B. McLaughlin, chairman of the Section, presided. F. R. Moulton presided over the Saturday afternoon joint session with Section E, the symposium on "Origin of the Earth."

The attendance at the Friday afternoon and Saturday forenoon sessions was the seating capacity of the room, or about 40. The attendance at the Saturday afternoon joint session was estimated as 150, about 100 seated and some 50 standing, with scores having been turned away.

In conclusion, it should be mentioned that the AAAS $1,000 prize awarded at each meeting for "a notable contribution to science" was presented to Harrison S. Brown for the paper "Elements in Meteorites and the Earth's Origin" (see *Science*, January 9, p. 36). (C. C. Wylie, *Secretary.*)

# *NEWS and Notes*

**The Association regrets to announce the death of Burton E. Livingston on Sunday, February 8, in Johns Hopkins Hospital. Dr. Livingston was elected part-time permanent secretary of the Association in 1920 and served in this capacity until 1930. The following year he became general secretary, continuing as such until 1934. From 1940 to 1943 he was chairman of the Executive Committee. His term as a member of the latter committee expired at the end of December 1946. The Association's work was advanced notably during the long period in which he served it in so many different ways.**

**A crystallization of vitamin K** on a microscope slide is portrayed on this week's cover. The crystals, prismatic rods, show twinning along the rod axis, the polarization colors changing abruptly within the crystals (shown by gray and white areas). The preparation was made in a purity determination in which the presence or absence of impurity is judged by the amount of liquid or solid foreign material observed as the vitamin K crystals grow from the melt. The appearance of the crystals, obtained in this manner, allows ready identification of vitamin K, its characteristics having been previously established.

The Armour Research Foundation is sponsoring a project, to be called the Crystallographic Center, the aim of which is to determine, check, and publish crystallographic data on a large number of chemical compounds in order that such identifications may be made. Over 100 prominent crystallographers, many of them from abroad, have indicated a desire to cooperate in the program, and with their aid the conventions for the descriptions are being established. Descriptions will appear regularly in *Analytical Chemistry,* beginning with the March issue. Completed data will be available on punched cards.

Further information may be obtained from W. C. McCrone, Jr., assistant chairman of Chemistry and Chemical Engineering Research, who is in charge of the program. A report on the progress of the Center since its establishment last September 1 will appear in the February 16 issue of *Chemical and Engineering News*. Plans are now being considered for a clinic on industrial applications of crystallography to be held at the Foundation in the summer of 1948.

## About People

**Carl C. Lindegren,** who has been associated with Washington University, St. Louis, since 1942, has been appointed research professor and chairman of the Department of Biological Science and Research, Southern Illinois University, Carbondale, where he will give a course on the genetics of microorganisms and continue his research on yeast genetics. This work is supported by grants from Anheuser-Busch, Inc., the U. S. Public Health Service, the American Cancer Society, and Southern Illinois University. Yeast cultures of different mating types are available for distribution to those interested.

Other appointments to the newly created department include Mrs. Lindegren and Mrs. J. S. Rafalko as faculty assistants and Bessie Kawachi as research assistant.

**Frederick R. Miller** has been appointed research professor of neurophysiology at the University of Western Ontario, London, Canada.

**H. J. Muller,** Department of Zoology, Indiana University, was elected in November to honorary foreign membership in L'Accademia Nazionale dei Lincei.

**Fordyce Ely,** professor of dairy husbandry, University of Kentucky, has been named chairman of the Department of Dairy Husbandry in the College of Agriculture, Ohio State University, to succeed **W. E. Krauss,** who became associate director of the Ohio Agricultural Experiment Station at Wooster on January 1. Dr. Ely will also be chief of the Department of Dairy Husbandry at the Experiment Station when its administration is merged with that of the College of Agriculture.

**James H. McGillivray,** who received his Master's degree in education from Wayne University in 1946, has gone to Bogota, Colombia, as assistant director of the U. S. Cultural Center there.

**Robert Meredith Page,** who will receive his Ph.D. degree in June from Harvard University, has been named assistant professor of biology at Stanford University, beginning with the fall semester.

## Colleges and Universities

**The University of Illinois College of Engineering** now has the largest faculty in its history, the addition last fall of nearly 150 new members bringing the total to 468. Appointments to the various professorial ranks included: as professor of physics, and dean of the graduate school of the University, Louis N. Ridenour; as visiting research professor in theoretical and applied mechanics, A. M. Freudenthal; as special research professor in ceramic engineering, W. R. Morgan; as professor of physics, A. T. Nordsieck; as part-time lecturer and research consultant in clay mineralogy in the Department of Civil Engineering, R. E. Grim, petrographer and principal geologist of the Illinois State Geological Survey; as associate professors, W. L. Hull, Everett Laitala, and L. C. Pigage (mechanical engineering), H. L. Langhaar (theoretical and applied mechanics), and Henry Quastler (radio-

biology, under physics); as assistant professors, G. F. Stockdale (ceramic engineering), L. G. Alexander (chemical engineering research), W. H. Munse, J. C. Dietz, E. R. Bretscher, and E. J. Daily (civil engineering), Thomas A. Murrell (electrical engineering), Harry Czyzewski (metallurgical engineering), and R. D. Hill and R. D. Rawcliffe (physics).

Additions to the teaching staff included: as associate and assistant professor, respectively, in aeronautical engineering, F. R. Stienbacher and Jacque Houser; as assistant professors in electrical engineering, D. S. Babb, J. R. Barkson, W. H. Byers, P. K. Hudson, J. P. Neel, and H. D. Webb; as assistant professor in mechanical engineering, R. N. McDonald; and as professor in theoretical and applied mechanics, N. O. Myklestad.

Other staff members added included instructors, research associates, research assistants, and assistants. Numerous promotions were also made in the various departments.

**Wayne University's Department of Chemistry** and the International Society of the Friends of the Kresge-Hooker Scientific Library are sponsoring a symposium lecture series entitled "Frontiers in Chemistry." The program follows: February 17, Harold C. Urey, University of Chicago, "Thermodynamic Properties of Chemical Substances"; February 23, Icie Macy Hoobler, Children's Fund of Michigan, "Nutrition and Chemical Growth in Childhood"; March 1, David E. Green, Medical School, University of Wisconsin, "Biochemistry From the Standpoint of Enzyme Chemistry"; March 8, Louis P. Hammett, Columbia University, "Structure and Reactivity of Organic Compounds"; March 15, Melvin S. Newman, Ohio State University, "The Behavior of Organic Compounds in Sulfuric Acid Solution"; March 22, K. C. D. Hickman, Distillation Products, Inc., "The State of the Surface During Distillation"; March 29, Morris S. Kharasch and Otto Reinmuth, University of Chicago, "The Importance of Free Radicals and Chain Reactions to Organic Chemistry"; and April 5, Hugh S. Taylor, Princeton University, "Molecular Structure in Fiber Systems." The lectures will be held at 7:00 P.M. in Room 314 of the main building at Wayne University. Graduate credit for the series is available to qualified persons desiring it, who should register as for any graduate course. Others interested may attend at a registration fee of $5.00 for the entire series. Requests for noncredit registration cards should be addressed to Neil E. Gordon, Department of Chemistry, Wayne University, and checks made payable to Wayne University.

## Meetings

**A Colloquium of Naval Medical Research** is to be conducted under the auspices of the Medical Sciences Division, ONR, at the Naval Medical Research Institute, Bethesda, Maryland, on February 19–21. According to Rear Adm. P. F. Lee, Chief of Naval Research, the colloquium is planned in order that the various scientists engaged in biological and medical research in more than 75 educational and research institutions under contracts with ONR may have an opportunity to see the nature of the studies supported and to gain clearer insight into the Navy's interest in fundamental research. Presiding officers at the various sessions include Detlev Bronk, Leonard Carmichael, William S. McCann, and Eugene DuBois. The 30 half-hour papers on the program include such various fields as respiration, microbiology, radiobiology, sensory physiology, Arctic studies, psychology, and dentistry. One afternoon during the program will be devoted to a tour of laboratories of the Institute.

**The National Council of Teachers of Mathematics** will hold its 26th annual meeting at the Claypool Hotel, Indianapolis, Indiana, April 2–3.

Among the frontier thinkers in mathematics education who will appear on the program are A. J. Kempner, University of Colorado; C. V. Newsome, editor, *American Mathematical Monthly;* Phillip S. Jones, University of Michigan; F. W. Kokomoor, University of Florida; Guy T. Buswell, University of Chicago; Raleigh Schorling, University of Michigan; W. W. Rankin, Duke University; F. Lynwood Wren, George Peabody College for Teachers; John R. Clark, Teachers College, Columbia University; Lois Knowles, University of Missouri; Philip Peak, Indiana University; Daniel W. Snader, University of Illinois; William Betz, Rochester Public Schools; Joseph A. Nyberg, Hyde Park High School, Chicago; Henry W. Syer, Boston University; Irene Sauble, Public Schools, Detroit; Mary Potter, Racine, Wisconsin; Virgil Mallory, Montclair, New Jersey; Rachel P. Kentston, Stockton High School, Stockton, California.

More than 50 topics are listed on the program. These range from "Elementary Arithmetic in an Activity Program" to "Coordinating High School and College Mathematics." Although topics concerning the philosophy of teaching mathematics have not been omitted, the emphasis seems to be on methods and special techniques in mathematics teaching. There will be a continuous showing of the latest visual aids in teaching mathematics.

Reservations may be made by writing directly to the Claypool Hotel, Indianapolis, Indiana.

## Elections

**The American Ethnological Society,** at its annual meeting on January 13 in New York, elected the following officers for 1948: Esther S. Goldfrank, Columbia University, president; Margaret Mead, American Museum of Natural History, 1st vice-president; Julian H. Steward, Columbia University, 2nd vice-president; Dorothy L. Keur, Hunter College, secretary-treasurer; and Marian W. Smith, Columbia University, editor. Burt W. Aginsky, College of the City of New York, Gordon Ekholm, American Museum of Natural History, and Carl Withers, New York City, were appointed directors.

**The Washington Academy of Sciences** has announced the following new officers: Frederick D. Rossini, president; C. Lewis Gazin, secretary; Howard S. Rappleye, treasurer; Francis M. Defandorf and William N. Fenton, members of the Board of Managers (to January 1951). Vice-presidents representing the affiliated societies are: Walter Ramberg, Philosophical Society of Washington; Wil-

liam N. Fenton, Anthropological Society of Washington; John W. Aldrich, Biological Society of Washington; Charles E. White, Chemical Society of Washington; C. F. W. Muesebeck, Entomological Society of Washington; Alexander Wetmore, National Geographic Society; William W. Rubey, Geological Society of Washington; Frederick O. Coe, Medical Society of the District of Columbia; Gilbert Grosvenor, Columbia Historical Society; Ronald Bamford, Botanical Society of Washington; William A. Dayton, Washington Section, Society of American Foresters; Clifford A. Betts, Washington Society of Engineers; Francis B. Silsbee, Washington Section, American Institute of Electrical Engineers; Martin A. Mason, Washington Section, American Society of Mechanical Engineers; Aurel O. Foster, Helminthological Society of Washington; Lore A. Rogers, Washington Branch, Society of American Bacteriologists; Clement L. Garner, Washington Post, Society of American Military Engineers; Herbert G. Dorsey, Washington Section, Institute of Radio Engineers; and Owen B. French, Washington Section, American Society of Civil Engineers.

**The Anthropological Society of Washington,** at its annual meeting in the U. S. National Museum on January 7, elected William N. Fenton as its president, W. Montague Cobb as vice-president, Marshall T. Newman as secretary, and John C. Ewers as treasurer. In addition, the Society elected E. Wyllys Andrews IV, Stella L. Deignan, George McC. Foster, William H. Gilbert, and Gordon R. Willey as councilors to the Board of Managers.

## NRC News

**The Committee for Research in Problems of Sex** has announced that requests for grants-in-aid during the fiscal period July 1, 1948–June 30, 1949, will be received until March 15, 1948. Application blanks may be obtained by addressing the Secretary, Division of Medical Sciences, National Research Council, 2101 Constitution Avenue, Washington 25, D. C. George W. Corner, director of the Department of Embryology, Carnegie Institution of Washington, Wolfe and Madison Streets, Baltimore 5, Maryland, is now chairman of the Committee. Robert M. Yerkes, organizer of the Committee and its chairman for 26 years, retired on June 30, 1947.

## Deaths

**Anthony Zeleny,** 77, emeritus professor of physics, University of Minnesota, died suddenly in Minneapolis on December 15.

**Johannes N. Brönsted,** 68, director of the Institute for Physical Chemistry, Copenhagen, Denmark, died on December 17. Prof. Brönsted was an honorary fellow of the New York Academy of Sciences and in April 1947 was elected a foreign associate of the National Academy of Sciences.

**Paul M. Rea,** 69, museum consultant, died January 15 in Santa Barbara, California. Dr. Rea had been engaged in private research since retiring as director of the Santa Barbara Museum of Natural History in 1937.

**Ernst Emil Herzfeld,** 68, archaeologist and professor emeritus in the School of Humanistic Studies of the Institute for Advanced Study, Princeton, died January 21 in Basle, Switzerland.

**T. D. A. Cockerell,** 81, professor emeritus of zoology, University of Colorado, died January 26 in San Diego, California, where he had gone to spend the winter.

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**The U. S. Army Medical Department** is planning extensive tests this spring of the efficacy of chloromycetin (*Science*, October 31, 1947, pp. 417–418) against the dreaded scrub typhus in the Far East. J. E. Smadel, director of virus research at the Army Medical Center and one of the discoverers of the drug, will fly to the Malay States with a supply for treatment of the native plantation workers among whom scrub typhus has a high mortality rate.

The proposed attack on scrub typhus is considered of great importance, as no existing vaccine has proved adequate. Early experiments with chloromycetin at the Army Medical Research and Graduate School demonstrated its potency against both typhus and scrub typhus microorganisms in experimental infections of incubated eggs and in animals. Dr. Smadel recently returned to Washington from Mexico City, where the new drug was tested against a small outbreak of typhus fever. Results of this test have not yet been announced.

**The Civil Service Commission** has announced a geologist examination to fill positions in the continental United States, as well as possibly a few positions in U. S. territories and possessions and in foreign countries. Entrance salaries are $2,644 and $3,397 a year. Full information and application forms may be secured from most first- and second-class post offices, from Civil Service regional offices, or from the U. S. Civil Service Commission, Washington 25, D. C. Applications must be received in the Commission's Washington office not later than March 2.

**The "annular" eclipse of May 8–9,** during which the moon will appear to be slightly smaller than the sun as it moves between it and the earth, will be a boon to mapmakers, in that it will enable a more accurate determination of the shape and size of the earth than has been made heretofore. Taking advantage of this particular eclipse, the National Geographic Society is sponsoring an expedition to 7 observation points. The Army Map Service, Engineers and Signal Corps, Navy, Air Force, National Bureau of Standards, Coast and Geodetic Survey, and the State Department will cooperate with the Society in the project.

The expedition scientists will measure with great precision the four times of contact between the edges of the sun and moon during the eclipse. These contacts will occur at different times at each of the 7 observation posts. By comparing the contact times at any two stations, accurate calculation of the distance between the two stations may be made, and their relative positions on the earth's surface located with an error of not more than 150 feet. In turn, these data can be used to determine very accurately the shape and size of the earth.

Expedition leaders and their observation posts are: E. A. Halbach, Milwaukee Astronomical Society, at Mer-

gui, Burma; Charles H. Smiley, Ladd Observatory, Brown University, at Bangkok, Siam, assisted by C. W. Miller, also of Brown University; Rev. Francis J. Heyden, Georgetown College Observatory, at Wu-k'ang, China, assisted by Y. C. Chang, a Chinese government astronomer; George Van Biesbroeck, Yerkes Observatory, at Cho-nan, Korea; a Japanese scientist to be stationed at Rebun Jima, Japan; and Lt. Cdr. Clarence R. Reed and Clarence A. Shelton, both of the U. S. Coast and Geodetic Survey, at Kiska and Adak. Each observation party will also include a radio technician, a camera technician, a surveyor, a cook, and a camp helper. American observers are expected to reach their eclipse stations about April 1.

**The U. S. Office of Education** has made its first two appointments of specialists in liberal arts at the college level: Jennings B. Sanders, former president of Memphis State College, as specialist in history, and Otis W. Freeman, on leave from Eastern Washington College of Education, as specialist in geography. The addition of other specialists in economics, psychology, the biological sciences, the physical sciences, and mathematics is also planned.

**The American Cancer Society** is sponsoring a new bimonthly journal, *Cancer*, for publication early this spring. Every phase of the cancer problem will be covered, with major emphasis on clinical aspects. In addition, the journal will publish a complete current bibliography of the world's cancer literature and comprehensive abstracts of significant articles. Fred W. Stewart, Memorial Hospital, New York, will serve as editor-in-chief and will be assisted by an Editorial Advisory Board of 50 authorities. Articles for Dr. Stewart's consideration should be addressed to 444 East 68th Street, New York 21, New York. Paul B. Hoeber, Inc., Medical Book Department of Harper & Brothers, 49 East 33rd Street, New York 16, New York, will publish *Cancer*. The yearly subscription price is $8.00.

**A quarterly journal** devoted to reports of original work in all branches of nutrition is currently being published by the Cambridge University Press. The journal, the first number of which appeared last September, is owned and controlled by the Nutrition Society and is entitled *The British Journal of Nutrition*. Following completion of Volume 5, the Proceedings of the Society, now published separately, will be incorporated into the journal. S. K. Kon, National Institute for Research in Dairying, Shinfield, nr. Reading, England, heads an Editorial Board of 12. The University of Chicago Press, 5750 Ellis Avenue, Chicago 37, is agent for the journal in this country. The price of Volume 1 is $15.00; single copies sell for $5.00.

**Seventeen expeditions are planned for 1948** by the Chicago Natural History Museum, it is announced by Clifford C. Gregg, director. Heading the list is the Bermuda Deep-Sea Expedition, a joint project with the Bermuda Biological Station for Research, Inc., of St. Georges, scheduled for June 1. The latter's participation will be supervised by its director, Dugald E. S. Brown, assisted by members of his staff. Leader and associate leader of this expedition will be Loren P. Woods and Fritz Haas, both of the Museum staff. Dredging to depths of from 8,000 to 12,000 feet, members of the expedition plan to collect fishes, squids, crustaceans, and other invertebrates for a projected new Hall of Deep-Sea Life featuring only those forms of life inhabiting the far deeps of the sea. In the opinion of Karl P. Schmidt, chief curator of zoology and expedition planner, this should be one of the most spectacular and interesting Halls in the Museum.

Based at St. Georges, the expedition will have its own ship manned by a crew of professional navigators and seamen. The 98-foot vessel, the *Caryn*, has been assigned to the Bermuda Biological Station by the Oceanographic Institution of Woods Hole and will be commissioned to sail for Bermuda in April.

Among the 16 other expeditions will be the 1948 Archaeological Expedition to the Southwest under the leadership of Paul S. Martin, chief curator of anthropology. In this 15th season of Southwest operations, Dr. Martin will conduct excavations of prehistoric Indian sites in New Mexico, assisted by a large staff of archaeologists and technicians.

A third expedition of interest will be the Rush Watkins Southeast Asia Expedition, leaving Chicago in September for Burma. Conducted by its sponsor, Mr. Watkins, Chicago businessman, and Austin L. Rand, the Museum's curator of birds, the expedition plans a comprehensive ornithological collection.

Fourteen other expeditions, some in participation with other groups, are scheduled by the Museum to Central and South America and Africa as well as points within the continental United States. Participating in these projects are: Colin C. Sanborn, curator of mammals; Rupert L. Wenzel, assistant curator of insects; Hugh C. Cutler, curator of economic botany; B. E. Dahlgren, curator emeritus of botany; Rainer Zangerl, curator of fossil reptiles; Melvin A. Traylor, Jr., associate; staff taxidermist Frank Wonder; Eugene S. Richardson, curator of invertebrate fossils; Robert Kriss Wyant, curator of economic geology; Harry Changnon, curator of exhibits; Sharat K. Roy, chief curator of geology; Paul C. Standley, curator of the herbarium; Frances Drouet, curator of cryptogamic botany; Philip Hershkovitz, assistant curator of mammals; and Harry Hoogstraal, assistant curator of insects.

## Make Plans for—

**National Educational Association,** winter meeting, February 21–25, Atlantic City, New Jersey.

**Optical Society of America,** winter meeting, March 4–6, Hotel Pennsylvania, New York City.

**American Association of Immunologists,** March 15, Atlantic City, New Jersey.

**American Physiological Society,** March 15–18, Convention Hall, Atlantic City, New Jersey.

**American Society of Ichthyologists and Herpetologists,** 28th annual meeting, March 26–29, Tulane University, New Orleans, Louisiana.

# *Comments and Communications*

## On Paraffin Embedding

I suspect that the reason Popham (*Science*, November 14, 1947, p. 475) is having so much trouble in his paraffin embedding is that he is trying to do it at a room temperature that is some 40° above the boiling point of his paraffin (!). Disconsidering these two obviously typographical errors, may I mention that all cavitations in paraffin embedding are a direct result of contraction of the paraffin in the center of the block after cooling at the borders first. This is purely a problem in physics, a study in coefficients of expansion. The trouble with all techniques of embedding in which the surface of the paraffin is blown upon to form a surface crust, followed by the immersion of the block, is that the block, now solidified at the surface, must still contract in its center. The obvious result is contraction spaces around the embedded object which interfere with its sectioning.

The solution to this problem is simply to fill the paper boat with hot paraffin (I use a temperature of 67°C), let the boat float upon water at room temperature until a thin film forms over the paper, let the specimen drop through the hot upper paraffin upon the congealed layer below, then allow the boat to cool slowly. The top will cool last, contracting as it does so, and concavities will not occur.

A. Weir Bell

*Los Angeles City College*

## Proposed Changes in Article 25 (the Law of Priority) of the International Rules of Zoological Nomenclature

At the International Zoological Congress held at Budapest in 1927, Article 25 of the International Rules of Zoological Nomenclature was amended by the addition of a third section (Section c), the provisions of which seemed so very reasonable and desirable that few taxonomists even yet have awakened to the nomenclatural confusion that may train from them. The paradox of provisions that can be characterized as reasonable and desirable and at the same time as potential sources of serious confusion and instability arises through their inclusion as mandatory parts of the Rules rather than as Recommendations, advisory in character. As mandatory parts of Article 25, there is a penalty for failure to comply, namely, "no generic name nor specific name published after December 31, 1930 shall have any status of availability (hence, also, of validity) under the rules, unless and until" the provisions of the new Section c are complied with.

In practice, a large percentage of the names proposed since 1930 appear to comply with the provisions of Section c, and with these names there are no difficulties. If the mandatory form of Section c has helped bring this about, as no doubt it has, that is to its credit. But there is still, and probably will continue to be, a significant percentage of names that are not proposed in compliance with Section c and hence, technically, are unavailable and invalid. For example, sampling indicates that something like 25% of the names proposed since 1930 to replace preoccupied names are invalid for failure to comply with the provisions of Article 25, c 2, as interpreted in Opinion 138. Nevertheless, few workers have recognized the fact. The invalid names enter into nomenclature, are employed again and again, and may become validated, perhaps inadvertently, somewhere in their history, but as of a different date and author.

If the penalty feature of Section c is ever generally recognized and a serious attempt to enforce it is made, great confusion will result, and a tremendous volume of otherwise useless bookwork will have to be undertaken. The penalty thus will not fall so much on the negligent author who earned it as on all subsequent workers who must deal with the name. Since the penalty features that arise through including the provisions of Section c as mandatory parts of the Rules are, in general practice, unenforced, and since their enforcement would create incalculable confusion, no time should be lost in removing these provisions from a mandatory status to an advisory one, from a section of Article 25 to the status of a Recommendation.

The Smithsonian Institution Committee on Zoological Nomenclature, composed of taxonomists of the U. S. National Museum, the Bureau of Entomology and Plant Quarantine, the Geological Survey, and the Fish and Wildlife Service, has considered what action might be taken and has approved the following suggestions for rewording Article 25 of the present Code and for Recommendations to accompany it.

It was the first thought simply to restore Article 25 to its original condition by removing the present Section c and setting up its provisions as Recommendations. Article 25 would then appear to have the advantages of brevity and succinctness—advantages, however, that are more apparent than real. For example, it took a long Opinion (Opinion 1) to interpret the word "indication" alone. Furthermore, some of the Opinions, notably 1 and 138, extend even farther the rigors of Article 25 and the evils that follow from unenforceable penalties. In addition, its coverage was incomplete in a number of important respects. Therefore, a complete rewording of Article 25 was undertaken and, with the accompanying Recommendations, is hereby laid before interested taxonomists in zoology and paleontology for discussion and constructive criticism. If it passes the tests of criticism and meets the approval of various organized groups to which it will be submitted, it is planned to send it to the International Commission on Zoological Nomenclature with the recommendation that it be passed on to the International Congress for consideration as an amendment to the Rules.

This amendment to Article 25, if adopted, will mark a deliberate halt to a dangerous trend that has been

growing more emphatic from decade to decade in the International Commission and reached a climax in Opinion 138 published in 1942. It is possible, if the trend is not reversed, that even more serious mistakes will be committed than the Budapest amendment and Opinion 138. The error is in trying to make the Articles of the Rules, which are theoretically mandatory, require optimum procedures rather than a necessary minimum, leaving the optimum a matter for Recommendations.

It should be stressed that the Rules can be enforced only through the voluntary action of the vast majority of working taxonomists. Taxonomists are only human, few are nomenclatural lawyers, and none are nomenclatural police. As a result, the penalty for failure to observe the rigorous provisions of the Budapest amendment and Opinions 1 and 138 is rarely enforced. The amendment proposed herein will cause little change in present practice and hence a minimum of confusion on the working level. It simply legalizes what is and always has been almost universal practice. At the same time, it is confidently expected that the Recommendations, which cover the provisions of Section c of the present Article 25 and which are couched in emphatic terms, will continue the good influences on nomenclatural practices which those provisions have had.

### *Suggested Form of Article 25 and Accompanying Recommendations*

"The Law of Priority"

Article 25 (Amended [date])

A. (1) The scientific name of the genus (generic name) is a single word (uninomial).

(2) The name that must be employed for a genus can be only that name which was first proposed for it in a publication on the condition

(a) that the name had not been used previously for another genus, and

(b) that the proposal of the name included

(1) a written description or diagnosis, or a reference to one previously published, or

(2) one or more species (named or unnamed) described, diagnosed, or illustrated in the same publication or a reference to such species in a previous publication or publications, or

(3) a reference to a preoccupied generic name that the name proposed was intended to replace.

B. (1) The scientific name of a species consists of two words (binomial) and has two components, the generic name (see A, above) which may be shared with many species and the specific name peculiar to that species within the genus.

(2) The specific name that must be employed for a species can be only that which was first proposed for it in a publication on the condition

(a) that the name complies with the provisions of Articles 35 and 36 on homonyms, and

(b) that the proposal of the name included

(1) a written description or diagnosis, or a reference to one previously published, or

(2) an illustration, or a reference to one previously published, or

(3) a reference to a preoccupied specific name that the name proposed was intended to replace.

C. The adoption of the above Article 25 nullifies and repeals the previous Article 25 and those Opinions or parts of Opinions heretofore adopted not in harmony with it, particularly Opinions 1 and 138. Names previously adjudicated in Opinions contrary to this article are to continue their adjudicated status but under Suspension of the Rules. Such names are hereby added to the official List under the Plenary Power.

*Recommendations:*

A. With respect to all names,

(1) Authors are urged to provide descriptions or diagnoses that are sufficiently complete to serve for effective recognition or differentiation of the genus or species from other genera or species. It is recommended also that species be illustrated.

(2) Authors are urged to give the references referred to above as definite and complete bibliographic references, at least once in the work in which the name is proposed. This reference should include the name of the publishing author, the indicated and actual year of publication (if they are different), the title of the paper or work and of the serial publication, and the number or numbers of the pages where the matter referred to appeared, and of any relevant illustrations.

B. With respect to generic names, it is extremely important that a type species be clearly and explicitly named as such in the original publication.

C. With respect to specific names, authors proposing a name are urged (1) to designate a single specimen (whether it be a complete organism or a part of an organism) as the type, preferably a figured specimen, and (2) to publish some means by which that specimen can be identified, such as a museum number or a distinguishing mark on the specimen or its label, and the name of the repository where it is preserved. The sole purpose of the type specimen is to fix the specific name on the species of which that specimen is an example, regardless of the taxonomic vicissitudes through which the name may pass.

EDWARD A. CHAPIN, J. BROOKES KNIGHT, and ROBERT R. MILLER

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## The Situation in Argentine Universities

The American Association of Scientific Workers has received from Buenos Aires a 64-page pamphlet, entitled "The Enslavement of the Argentine University (Avasallamiento de la Universidad Argentina)," recently issued by the Federation of Societies for Defense and Advancement of the Democratic and Free University. Reprinted in the pamphlet is the declaration adopted by the Federation at its organization meeting last year:

The Federation of Societies for the Defense and Advancement of the Democratic and Free University, having formed itself in the city of Rosario, on December 15, 1946, addresses itself to the public of the Nation and to the universities of America, to make known to them the state of the Argentine universities and the reasons which determine our position. We affirm that the universities have been enslaved by essentially political intervention; that they now lack the climate and urge for research, respect for freedom, or interest in the true creative and moral capacities of the youth. We affirm that the universities are now in process of transformation from their free, democratic status into another type, purely totalitarian.

In the face of the above facts, which have resulted in the loss and dispersion of the teaching staffs, with grave consequences for the national culture; in the face of systematic persecution of teachers and students, deprived of all rights and guarantees of law and with their representative socie-

ties dissolved, we affirm that life in the universities is at present incompatible with the dignity of man and of the teacher.

Declaring ourselves united for the common ideal of the University; for loyalty purely to democracy; for the wholehearted support of culture; for conscious adherence to the imperatives of the intellect; and for complete solidarity with the people in their striving against privilege and oppression, we shall fight to wed science and labor in a free, democratic University, dedicated to the service of the people.

As Virginia Lee Warren reports ("Peron Widens Rule Over Universities," *New York Times*, December 22, 1947), "Since May 2, 1946, the formerly autonomous institutions once . . . by far the best in Latin America, have been run by the Government through 'interventors' appointed by the Executive power. Now, . . . they are theoretically being turned back to their own administrations. But since the new law gives the President of the nation the right to appoint the head of each university and thus, in effect, to pass upon all faculty appointments, the Government will still be in complete control."

The preface of the pamphlet outlines the present situation in the Argentine universities:

During the current political crisis, for our country the major one in 50 years, the university professors of Argentina, responding honorably to the democratic tradition and to the demands of citizenship, have opposed themselves firmly to the advances of a dictatorship which arose from the militarist movement of 1943. In doing so, they complied with their civic duties, inescapably imposed upon them by their position as teachers of the youth. It was not only the professors who raised their voice against the dictatorship. The students of all the Argentine universities responded similarly in the defense of the basic institutions of the country, gravely endangered at this sad hour of our political evolution.

The Decree of Intervention, pronounced in May, 1946, was an act of vengeance against this patriotic attitude; its aim was to dismiss from their posts all who, maintaining their duties as teachers, also complied with their civic duties. The Government dismissed all the Rectors, the great majority of the Deans, and a large number of Professors, whose lives had been dedicated to teaching and to scientific research. No consideration was given to, nor respect shown for, the prestige which the work of these savants had brought to Argentine science.

Through the vindictiveness of the Government, the students of Argentina have received the most impressive lesson of their careers; but happily for the future of the country, they have protected and cherished their high ideals without equivocation. In all manner of means they have expressed their respect and regard for their dismissed professors, who had undertaken their civic and patriotic activities only because of high moral concepts and at risk to their personal interests. Many students have been imprisoned, suspended, or dismissed; some have had to continue their studies out of the country, finding it impossible to do so in the halls of the Argentine universities.

Time cannot erase the humiliation which has been heaped, not on the professors themselves, but really on the dignity of the university professions. Despite the formal return to constitutional law, the universities have undergone actually no change either with respect to acts of the Government, or the decisions of professors and students. The Government maintains the Argentine universities in the same state of vassalage; students and professors continue their protest against this situation.

Through the imposition of the new laws, the Free University will disappear, and with it justice and right for the professors, freedom to teach, and stimulus to investigators who will never feel secure in their classrooms and laboratories. With the dismissal of the group of professors who gave lustre to the Nation's universities and examples to the youth, and with authority, through the new laws in other hands, the universities will have ceased to be fountains of learning and will have become mere technical schools and mouthpieces for the bureaucracy of the Government.

This pamphlet aims to let the Argentine people, and all those of the universities of the world who read it, know how the free universities of Argentina have been enslaved and no longer respect the rights of those who have dedicated their lives to the task of educating younger generations. The autonomy of the universities has been lost; they are entirely at the mercy of the Government, with their very life and activities subjected to the yoke of politicians.

The names recorded here tell eloquently of the profound change that has befallen our national universities; of the attack on men of science, on science itself, on culture in general, and on the right of all citizens to express publicly their opinions and ideas.

Subscribed to this statement are the names and faculty positions of 1,073 former members of the 6 Argentine universities. The number includes those who were dismissed outright, "retired" against their will, or who resigned in protest against these acts of the Government. Among them are the most famous names of Argentine scholarship, including such men as Bernardo A. Houssay, Nobel Prize winner for 1947, "retired" a year ago at the age of 55; the Spanish scholar, Amado Alonzo, dismissed and now at Harvard; and Ricardo Rojas, world famous historian, resigned and now a member of the Opposition in the Chamber of Deputies. The data are summarized below.

| *University* | *Dismissed* | *"Retired"* | *Resigned* | *Total* |
|---|---|---|---|---|
| Buenos Aires | 34 | 17 | 98 | 149 |
| Cordoba | 56 | 9 | 228 | 293 |
| Cuyo | 36 | 5 | 5 | 46 |
| La Plata | 121 | 23 | 75 | 219 |
| Litoral (Rosario) | 87 | 4 | 221 | 312 |
| Tucumán | 51 | ..... | 3 | 54 |
| | 385 | 58 | 630 | 1,073 |

A memorandum on these events was submitted by the American Association of Scientific Workers to Secretary of State Marshall on March 5, 1947, together with data on current persecutions of scientists in Greece and Portugal. Thus far no action appears to have been taken by our Government.

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# TECHNICAL PAPERS

## A New Penicillin for Prolonged Blood Levels[1]

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The advantages of prolonged therapeutic blood levels of penicillin following a single injection have been well proven with penicillin in oil and wax (Romansky formula) (*3–6*). However, the necessity of wax in this vehicle has been the apparent cause of a number of unfavorable local reactions.

In our laboratories, a sparingly soluble penicillin has been developed which may be administered in various vehicles, such as oil, oil and water emulsion, or physiological saline. This new crystalline penicillin complex was produced by chemically combining procaine with penicillin G. These crystals have a solubility of approximately 7,000 units/cc in the above vehicles. They contain not less than 90% penicillin G, with a potency of 940 units/mg (1,040 units/mg, theoretical). In the oil suspension, at least 50% of the total weight of the particles are 50 μ or more in length.

The importance of particle size for the slow absorption of penicillin has been demonstrated by Dowling, *et al.* (*1*) and Welch, *et al.* (*7*). In preliminary experiments with laboratory animals, it was shown that the administration of a suspension of procaine penicillin crystals resulted in the slow release of penicillin G over prolonged periods. The blood levels reported in Table 1 were obtained in 21 hospitalized patients receiving the material suspended in cottonseed oil. Blood level determinations were made, using Kolmer's method.

TABLE 1

BLOOD LEVELS OF PROCAINE PENICILLIN IN COTTONSEED OIL*

| Patient | Date | Amount administered (cc)† | Blood levels | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | 1 hr | 2 hrs | 12 hrs | 18 hrs | 24 hrs | 36 hrs | 48 hrs |
| 1 | 8–17–47 | 1 | | 0.496 | | 0.124 | 0.248 | | |
| 2 | 8–18–47 | 1 | 0.248 | | 0.124 | 0.124 | 0.124 | | |
| 3 | 8–18–47 | 1 | 0.992 | | 0.248 | 0.124 | 0.124 | | |
| 4 | 8–18–47 | 1 | 0.496 | | 0.248 | 0.124 | 0.124 | | |
| 5 | 8–19–47 | 1 | | 0.124 | 0.124 | 0.124 | 0.124 | | |
| 6 | 8–19–47 | 1 | | 0.124 | 0.124 | 0.062 | 0.062 | | |
| 7 | 8–20–47 | 1 | 1.984 | | 0.124 | 0.062 | 0.062 | | |
| 8 | 8–20–47 | 1 | 0.248 | | 0.124 | 0.062 | 0.062 | | |
| 9 | 8–24–47 | 1 | 0.062 | | 0.062 | 0.062 | 0.062 | | |
| 10 | 9– 3–47 | 1 | 0.496 | | 0.124 | 0.062 | 0.062 | | |
| 11 | 9–15–47 | 1 | 0.248 | | 0.496 | 0.248 | 0.124 | | |
| 12 | 9–15–47 | 1 | 0.496 | | 0.248 | 0.031 | 0 | | |
| 13 | 9–16–47 | 1 | 0.062 | | 0.124 | 0.124 | 0.062 | | |
| 14 | 9–16–47 | 1 | 0.992 | | Not drawn | 0.124 | 0.124 | | |
| 15 | 9–17–47 | 1 | 0.496 | | 0.248 | 0.124 | 0.062 | | |
| 16 | 9–17–47 | 1 | 1.984 | | 3.968 | 0.496 | 0.124 | | |
| 17 | 9–20–47 | 1 | 0.824 | | 0.824 | 0.659 | 0.329 | | |
| 18 | 9– 3–47 | 2 | 0.124 | | 0.124 | 0.124 | 0.124 | | |
| 19 | 9–16–47 | 2 | 1.984 | | 0.496 | 0.496 | 0.062 | | |
| 20 | 9– 8–47 | 3 | 0.992 | | 0.248 | 0.248 | 0.248 | 0.031 | 0 |
| 21 | 9– 8–47 | 4 | 0.496 | | 0.496 | 0.496 | 0.496 | 0.248 | 0.124 |

* Control blood level determinations on all patients were 0.
† 1 cc contains 300,000 units.

It will be noted that in only one individual were there blood levels lower than 0.062 at the 24th hr. Many of the patients showed levels of 0.124. One patient receiving 3 cc (900,000 units) had a therapeutic blood level at the 36th hr, and a second patient receiving 4 cc (1,200,000 units) had a blood level of 0.124 at the 48th hr. In none of the cases were there any local or systemic reactions.

[1] Since submitting this manuscript for publication, it has been brought to our attention that Charles J. Salivar, F. H. Hedger (Chas. Pfizer and Co., Inc.), and E. V. Brown (Department of Chemistry, Fordham University) have in the press (Note to the Editor Section, *Journal of the American Chemical Society*) a manuscript dealing with the chemical characterization of the procaine salt of benzyl penicillin.

Further studies are now being conducted, using various other vehicles.

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## Delayed Maturity of Bean Plants Sprayed With Solutions of 2,4-Dichlorophenoxyacetic Acid of Nonherbicidal Concentrations[1]


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In a study (*2*) of the effect of 2,4-D on the growth and development of bean plants it was found that their maturity was markedly delayed by concentrations of 10 ppm applied just prior to flowering. Since this response from 2,4-D has not been found in the literature and since it is thought to be of practical interest, a report on the conditions under which it was observed is presented here.

A certified strain of red kidney bean was used in experiments which were carried out under greenhouse conditions during the spring and under field conditions during the summer. The plants were sprayed when the first trifoliate leaf was expanding, using an aqueous solution of a commercial herbicide (70% 2,4-D and 30% sodium bicarbonate) acidified with 1 gm of citric acid/liter to increase activity (*1*). The solutions were applied as a fine mist by means of an atomizing nozzle, the leaves being wet on both sides.

In the greenhouse study, a stand of uniform plants was obtained in pots containing equal amounts of soil and at intervals receiving an equal amount of a dilute, complete nutrient solution. On April 7 (1 week after germination) half the plants were sprayed with a solution of 2,4-D at 10 ppm. The treatment caused slight epinasty of stem and petioles, and inhibited temporarily the rate of top growth, while the root growth seemed to be accelerated.

Flowering began about May 1. At this time the top growth of treated plants was as vigorous as that of untreated plants, although the former were somewhat greener, and some of the younger leaves of the treated plants showed toxicity symptoms. Affected leaves became dark green, dwarfed, and lanceolate and felt thicker and stiffer than leaves on untreated plants. By the end of May there was a definite, more vigorous growth of treated than of untreated plants. At harvest time, the middle of June, the treated plants carried a significantly higher number of leaves per plant (an average of 11.2 leaves per treated plant and only 6.6 leaves per untreated plant). This difference reflects the more extensive lateral growth of treated plants. Fig. 1 shows representative plants, treated and untreated, from which all leaves and petioles were removed to indicate this development of lateral branches from the second node of the treated plants.

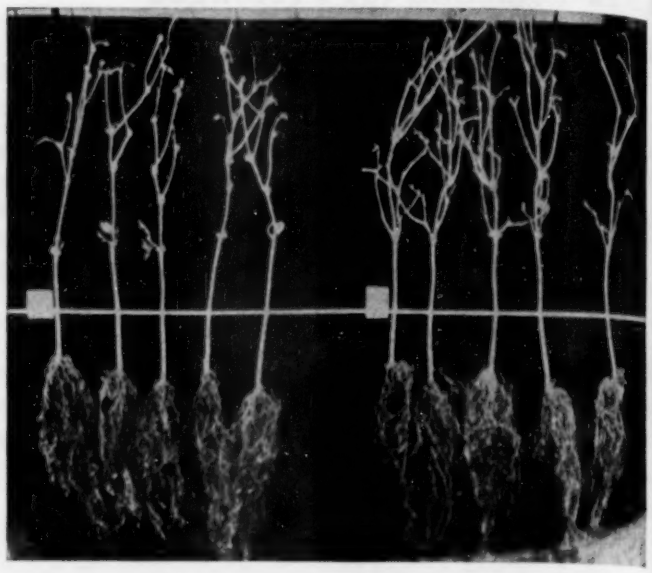

Fig. 1. Development of lateral branches from the second node of plants treated with 2,4-D at 10 ppm just prior to flowering (*right*) as compared with untreated plants (*left*).

TABLE 1

Average Number and Weight per Plant of Pods and Seeds of 14 Untreated Plants and 12 Plants Sprayed With a Solution of 10 ppm of the Sodium Salt of 2,4-D

| Plants | No. of pods | Wt. of pods (gm) | No. of seeds | Wt. of seeds (gm) |
|---|---|---|---|---|
| Untreated ..... | 3.2 | 9.6 | 10.2 | 7.4 |
| Treated ..... | 4.1 | 10.6 | 11.6 | 7.6 |

On June 2, counts showed a greater set of fruits on treated than on untreated plants. Of fruits which actually developed, however, there were not significantly more on treated than on untreated plants (Table 1).

While the untreated plants turned yellow at the beginning of June, the treated ones remained green for about two weeks longer. The pods were not harvested until

[1] Journal article No. 911 (m.s.) of the Michigan Agricultural Experiment Station.

they were completely dry (Table 2). The time of delayed maturity amounted to almost two weeks.

TABLE 2

NUMBER OF FULLY RIPE PODS, ON DAYS INDICATED, FROM 14 UNTREATED PLANTS AND 12 PLANTS SPRAYED WITH A SOLUTION OF 10 PPM OF THE SODIUM SALT OF 2,4-D

| Plants | June 14 | June 23 | June 28 | Total |
|---|---|---|---|---|
| Untreated ..... | 45 | 0 | 0 | 45 |
| Treated ..... | 7 | 30 | 12 | 49 |

In the field experiment, seed which was selected for uniformity was planted May 31. On June 24, treatments were made with solutions of 1, 10, and 100 ppm of 2,4-D, using a system of randomized blocks and 5 replications. There were 20 plants in each plot.

TABLE 3

NUMBER OF PODS HARVESTED AT DATES INDICATED FROM BEAN PLANTS SPRAYED WITH SOLUTIONS OF DIFFERENT CONCENTRATIONS OF THE SODIUM SALT OF 2,4-D

| | August | | | September | Total yield | Relative yield |
|---|---|---|---|---|---|---|
| | 18 | 21 | 27 | 3 | | |
| Control .... | 282 | 423 | 742 | 63 | 1,510 | 100 |
| 1 ppm ... | 198 | 505 | 917 | 89 | 1,709 | 113* |
| 10 ppm ... | 138 | 564 | 920 | 64 | 1,686 | 111 |
| 100 ppm ... | 0 | 72 | 633 | 685 | 1,390 | 92† |

* No significant increase as compared to control.
† No significant decrease as compared to control.

The day after the treatments, the plants sprayed with 100 ppm showed pronounced epinasty, while only slight epinasty was observed at 10 ppm, and none at 1 ppm. The growth of the plants sprayed with 100 ppm was markedly inhibited and, even though the plants recovered, they never became as vigorous as those in any of the other plots. At 10 ppm there was observed only a slight decrease in top growth for a short period, after which the plants became as vigorous as those untreated. No effect upon the growth was observed from treatments with 1 ppm.

TABLE 4

PER CENT OF TOTAL NUMBERS OF PODS HARVESTED AT DATES INDICATED FROM BEAN PLANTS SPRAYED WITH SOLUTIONS OF DIFFERENT CONCENTRATIONS OF THE SODIUM SALT OF 2,4-D

| | August | | | September |
|---|---|---|---|---|
| | 18 | 21 | 27 | 3 |
| Control ..... | 19 | 28 | 44 | 9 |
| 1 ppm ..... | 20 | 30 | 54 | 4 |
| 10 ppm ..... | 8 | 33 | 55 | 4 |
| 100 ppm ..... | 0 | 5 | 45 | 50 |

By the beginning of July, abnormal leaves developed in the plots sprayed with 100 ppm, 5–10 leaves on the lower lateral branches showing formative effects. In the plots sprayed with 10 ppm, only a few plants developed abnormal leaves.

In the middle of July, flowering commenced in the control plots and in plots sprayed with 1 and 10 ppm. In the 100-ppm plots, however, flowering was markedly delayed, although by the end of July the growth seemed to be somewhat accelerated, no more abnormal leaves developed, and flowering was initiated.

Harvesting of all fully matured pods was made at four different times as indicated in Table 3. Table 4 gives the distribution of the total yield as per cent on the different dates, showing that treatment with 2,4-D at 100 ppm markedly delayed maturity, with some slight decrease in number of pods and yield of seed.

The changes in the metabolism of the plant brought about by the treatment with 2,4-D seem to have a secondary effect on the axillary growth and the time of maturity of the plant. Weaver (*3*) mentions delayed and decreased pod production after treatments with 2,4-D. The opposite effect, namely, hastening of maturity, is reported by Wittwer and Murneek (*4*), who sprayed snap beans in the flowering stage with different growth substances including 2,4-D. This difference in response may be due to differences in age of plants and the parts involved. In the case of snap beans the effect seems to be a direct one on the growth of the ovary, while in the present study the effect is an indirect one following stimulated axillary growth.

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## Survival Time of Various Warm-blooded Animals in Extreme Cold


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Little is known about the resistance of homoiothermic animals to low temperatures. This paper reports some observations on the survival time and body temperature of various adult animals at an ambient temperature of −35° C.

The experiments were conducted over a four-day period in which the temperature of the cold room varied between −34° and −37° C. Previous determinations by Belding, *et al.* (*1*) had shown the wall temperature of this room

[1] Operated as a function of the Environmental Protection Section, Research & Development Branch, Quartermaster Corps. These experiments were carried out in the Cold Room of the Harvard Fatigue Laboratory; the assistance of its director, W. H. Forbes, is gratefully acknowledged.

to be within ±1° of the ambient air temperature and the air movement to be turbulent but continuous at a rate of about 2 m.p.h.

TABLE 1

| Species | No. of animals | Average weight (gm) | Local frostbite noted | Survival time (hrs) |
|---|---|---|---|---|
| Mouse, Carworth CF1 strain | 10 | 19 | Feet & tails | 0.4 |
| Canary | 2 | 20 | | 0.6 |
| Rat, Wistar | 7 | 242 | Feet & | .75–1.6 |
| albino | 5 | 227 | tails | 2.0 |
| Rabbit, New | 3 | 1,760 | Ears | 3.5–5.0 |
| Zealand white | 6 | 1,774 | | 5.0–6.5 |
| Chicken, white | 8 | 1,642 | Combs | 3.3–16.0 |
| Leghorn | 3 | 1,537 | & feet | 16.0–29.5 |
| Pigeon, Army | 5 | 395 | Feet | 22–24 |
| carrier | 4 | 397 | | 24–48 |
| | 2 | 408 | | 48–78 |

The animals were confined, one at a time, in metal cages with cardboard bottoms. The rectal temperatures of rats and rabbits and the cloacal temperatures of pigeons and chickens were measured either by thermometer or by copper-Constantan thermocouples read with a potentiometer. In a few cases the animals were

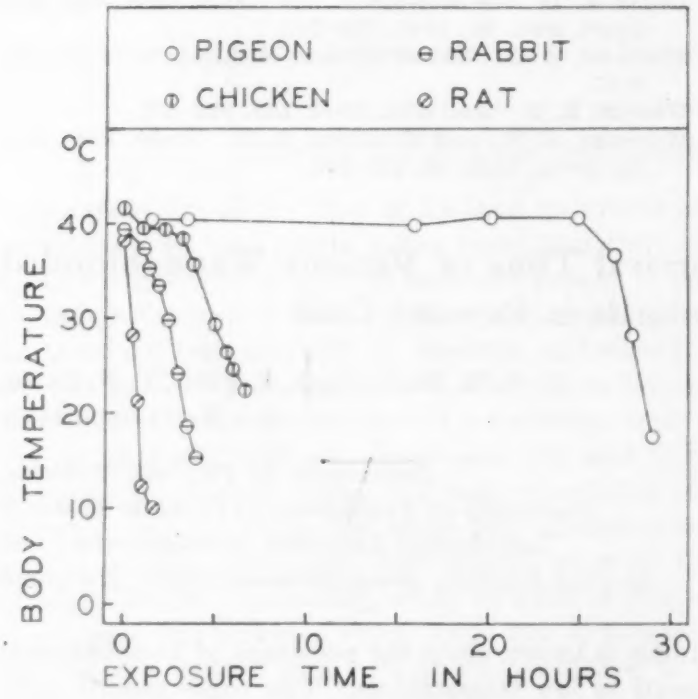


FIG. 1

removed from the cold room into a cool room for insertion and reading of the rectal thermometer. Although the animals had room for movement in their cages, they sat quietly in one place and exposed a minimum of surface. They were not fed during the experiment.

A summary of the results is given in Table 1. Body temperature for representative individuals is plotted against time in the cold in Fig. 1. The last temperatures recorded were obtained on moribund animals. The data indicate that the adaptability to low environmental temperatures varies considerably among species as well as within a species. The pigeon appeared to be especially well adapted to cold, one pigeon surviving without food for 78 hrs in an ambient temperature of −35° C. Local frostbite was noticed on the ears of rabbits, on the combs of chickens, on the feet of chickens, pigeons, mice, and rats, and on the tails of rats and mice.

Our observations are in accord with the experiments of Giaja (*2*), who found the lowest temperature at which the animal can maintain its body temperature for one hour to be: for the pigeon, −85° C; for the chicken, −50° C; for the rabbit, −45° C; and for the white rat, −25° C. Giaja's experiments in which environmental temperature was the variable and our experiments in which time was the variable placed species in the same order in regard to the degree of their resistance to cold.

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## The Respiration of *Streptomyces griseus*


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Since the presence of air is necessary both for the growth of *Streptomyces griseus* and for the production of streptomycin, an investigation of the respiration of this organism was pertinent. The experiments were designed to determine (1) oxygen consumption during the growth of the culture, (2) its relationship to antibiotic production, and (3) whether a correlation exists between the abilities of different strains to produce the antibiotic and their different rates of respiration.

Strain A2, which was most generally used in these studies, was derived as a single colony isolate from *S. griseus* H9 received from S. A. Waksman. Other strains such as 4c5 and R.M. 1067 were obtained by irradiating A2 with ultraviolet light. All cultures were grown in 500-ml Erlenmeyer flasks containing 125 ml of a modified streptothricin assay medium and were agitated on a reciprocal shaker. Assays for streptomycin were made according to the method described by Loo, *et al.* (*2*), and the growth of the organism was based upon the dry weight of washed and centrifuged mycelium. Studies of oxygen consumption were made in a Warburg apparatus using the techniques described by Umbreit, Burris, and Stauffer (*3*). Fifteen-ml aliquots of the culture were used for determinations of mycelial weight and 2 ml for respiration studies. When dextrose was added to the medium during measurements of oxygen consumption,

this was done from the side arm, and a similar amount of water was added to the control flasks. All analyses were made on samples taken from triplicate flasks at various periods during the growth and decline of the culture.

TABLE 1

GROWTH, RESPIRATION, AND PRODUCTION OF STREPTOMYCIN OF *S. griseus* A2

| Age of culture (hrs) | Wt. of mycelium (mg) | $O_2$ consumed (μl) | Mycin produced (units/ml) |
|---|---|---|---|
| 0 | 0.7 | 32 | 0 |
| 12 | 4.8 | 344 | 0 |
| 24 | 11.2 | 281 | 50 |
| 36 | 12.2 | 127 | 188 |
| 48 | 10.2 | 63 | 227 |
| 60 | 8.1 | 40 | 223 |
| 72 | 6.9 | 30 | 211 |
| 84 | 6.6 | 30 | 201 |
| 108 | 6.6 | 30 | ... |

The growth of *S. griseus* followed the general pattern which has previously been described (*2*): the weight of the mycelium increased until about 36 hrs after the flasks were inoculated, then steadily declined (Table 1). Respiration followed a different pattern. During the early period of growth, respiration was at its highest level; thus, in one experiment this occurred when only 39% of the maximum growth had been obtained (Table 1).

TABLE 2

RESPIRATION OF *S. griseus*

| Age of culture (hrs) | $QO_2$ | $QO_2$ when dextrose added | Age of culture (hrs) | $QO_2$ | $QO_2$ when dextrose added | Age of culture (hrs) | $QO_2$ | $QO_2$ when dextrose added |
|---|---|---|---|---|---|---|---|---|
| 0 | 32 | ... | 0 | 29.5 | ... | 0 | 46.0 | ... |
| 19 | 40.5 | 45.2 | 15 | 41.4 | 45.1 | 12 | 71.8 | ... |
| 45 | 12.6 | 22.2 | 39 | 10.7 | 14.8 | 24 | 25.3 | ... |
| 69 | 7.5 | 19.0 | | | | 36 | 10.3 | ... |
| 93 | 3.8 | 0 | | | | 48 | 6.2 | ... |
| | | | | | | 60 | 5.5 | 13.0 |
| | | | | | | 72 | 4.3 | 9.1 |
| | | | | | | 84 | 4.6 | 11.7 |
| | | | | | | 108 | 4.0 | 7.9 |

Respiration was then gradually reduced during the remainder of the growth and decline of the culture. This can best be noticed when the $QO_2$ is compared at various periods of growth (Table 2). Both total oxygen consumption and the oxygen consumption per unit weight of mycelium decreased during the period in which the total amount of the *S. griseus* present in the culture was increasing. This diminution of oxygen demand continued into the decline phase, when the total weight of the fungus was steadily reduced. Streptomycin was not produced in the culture until about 12–24 hrs of growth and reached a maximum somewhere between 48 and 60 hrs. Greatest antibiotic production therefore occurred during the period of low oxygen consumption and was not detected during the early period when respiration was at its highest level.

The decrease in oxygen consumption during the growth of *S. griseus* could be due to two factors: (1) a depletion of sugar in the medium with a concomitant reduction in metabolism, and (2) an inherent change in respiration of the cells with age. The addition of dextrose to the medium should then result in an increased respiration, if depletion of the media was the main factor in decreased respiration. Though some increase in respiration occurred when the dextrose concentration was increased, the increase never reached the levels of respiration of the very young culture (Table 2). It is apparent, then, that two phenomena were involved in the change in oxygen consumption of *S. griseus*. One of these factors is the very high rate of respiration of the young mycelium. Synthetic metabolism was at its highest level during this period. In the first 12 hrs the mass of mycelium increased 6.5 times, whereas in the next two consecutive like periods the increase was only 2.3 and 0.09 times, respectively. Oxygen consumption was related to synthetic ability during these periods, since the changes in the $QO_2$ of the mycelium follow the changes in growth rate in various stages of the culture's development. During the period of decrease in the weight of mycelium present in the culture, the reduced respiration was expected, though some oxygen might have been utilized in some of the enzymatic processes of autolysis. The other factor responsible for reduced respiration was the amount of dextrose removed from the solution by the growing culture. This process has been shown to be very rapid in young cultures (*1*). Though increases in oxygen consumption occurred on the addition of dextrose, the fact that it never reached the level of the young cultures indicates that both the age of the culture and the dextrose concentration are involved in the respiration fluctuations which occur in shake culture.

The ability of different strains of *S. griseus* to produce streptomycin could not be correlated with their rates of respiration. Strain R.M. 1067, which produces no detectable amounts of streptomycin, had a $QO_2$ of 51.9, whereas A2, which produced 240 units/ml had a $QO_2$ of 23.3. Another strain, 4c5, which produced even more streptomycin (350 units/ml) had a $QO_2$ of 61.0. No discernible relationship therefore exists between oxygen consumption and streptomycin production either at various stages in the culture of the fungus or between strains of different synthetic capacities.

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# Sucrose in the Stalks of Maize Inbreds

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A Connecticut field corn inbred, C103, has been discovered to have sucrose in the juice of the stalks in an amount approaching that of sugar cane. A sample of juice extracted on October 18, 1947, contained 8.65% sucrose and 2.60% invert sugar, or a total of 11.25% sugar. The juice of normal cornstalks varies in sucrose content from 1 to 4%.

The stalk of C103 has appreciably more vascular bundles than the normal corn, with very little pith and a rather large proportion of juice. A similarity was noticed between this stalk and that of sugar cane observed growing in Cuba more than a score of years ago. The recollection led to tasting the C103 stalk, and, surprisingly, it seemed almost as sweet as cane. Chemical analyses of the cornstalks confirmed the sweetness and demonstrated that the sugar present was largely sucrose.

TABLE 1

| Sample No. | Inbred or hybrid | Invert sugar (%) | Sucrose (%) | Total |
|---|---|---|---|---|
| 499 | C103 | 1.80 | 5.72 | 7.52 |
| 500 | 2477 Tr (Ldg) | 2.62 | 3.52 | 6.14 |
| 503 | 1948 Tr (Ldg) | 3.48 | 2.70 | 6.18 |
| 504 | 1951 L317 | 1.68 | 2.91 | 4.59 |
| 498 | C102* | 2.32 | 3.45 | 5.77 |
| 501 | 2478 C102 × C103 | 1.68 | 1.37 | 3.05 |
| 502 | 2488 L317 × C103 | 0.74 | 1.27 | 2.01 |

* Ears were harvested from this sample about 10 days before sample was taken. Consequently, this figure is probably too high in comparison with the others.

TABLE 2

| Sample No. | Inbred or hybrid | Invert sugar (%) | Sucrose (%) | Total |
|---|---|---|---|---|
| 679 | C103–2 | 2.60 | 8.65 | 11.25 |
| 678 | C103 | 3.57 | 6.20 | 9.77 |
| 681 | Tr (Ldg) | 5.83 | 2.11 | 7.94 |
| 680 | C103 × Oh 40B* | 1.93 | 9.27 | 11.20 |
| 682 | Syrup C103 | 20.32 | 35.99 | 56.31 |

* Stalks had no ears. Consequently, this figure is higher than for an average of this hybrid, which seemed to vary considerably in sweetness when the stalks were tasted.

The occurrence of sucrose in cornstalks has long been known. Before the Spanish Conquest of Mexico, the Aztecs made sugar from the stalks of Indian corn, *Zea Mays* L. (*3*). Also, various attempts, some almost 100 years ago, were made by the U. S. Department of Agriculture (then a division of the Patent Office) to secure sugar commercially from cornstalks (*2*). A patent was taken out by F. L. Stewart in 1906 for a method of increasing the sugar content of cornstalks. This method consisted in removing the ears before maturity, thus doubling the amount of sugar in the stalks. A report of experiments along this line is given by Clark (*1*). After a rather thorough investigation it was concluded that it was not economically feasible to use cornstalks as a source of sugar even though sugar could be obtained.

This conclusion was undoubtedly correct for open pollinated corn, where one has only limited control over the heredity. But with the pure-line method it is quite a different story. Pure lines can be maintained, and undoubtedly the sugar content can be increased by breeding for this specific character. The stalks which were analyzed had, for the most part, produced good ears. Thus, it should be possible to incorporate the high sugar content into the commercial field corn hybrids, harvest the ears, and have the sugar crop as a by-product.

Tables 1 and 2 show, respectively, the amount of sugar found in the stalks and the juice.

Since the sweetness of C103 was not discovered until October 1, few analyses were made in 1947. Although the leaves were killed by frost in late September, a sample of C103 taken on October 18 showed as high a percentage of sucrose as was found previously. Thus, there would be some time for processing stalks after harvesting the ears.

Two hybrids, L317 × C103 and C102 × C103, showed a very low percentage of sugar, although both had a stalk similar to C103. Thus, it seems that the sugar content is not a function of the same gene or genes causing the stiffer stalk. The stiff stalk behaves as a dominant and the sugar content as a recessive. The hybrid C103 × Oh 40B had a high sugar content, but this was probably partially caused by having produced no ears. The stalks on which ears were produced did not have a high juice content and were not sweet to the taste. Neither was the Ohio inbred Oh 40B sweet to the taste.

While the high sucrose content of the cornstalks may make sugar production from corn feasible, it may also render cornstalks higher in feeding value for farm animals. This should be true not only for silage but when the stalks are fed green. The higher sugar content might also cause the stalks to be more palatable.

The high sucrose of cornstalks at present is confined to one inbred line, C103. Manufacture of sugar on a commercial scale, or utilization of the higher sugar content for feeding animals, must await the development of a hybrid with stalk juice high in sugar. To produce such a hybrid, one or more inbreds similar to C103 in genetic constitution will be needed. If these cannot be discovered among existing ones, it will be necessary to develop them by breeding—a feasible project but one requiring time.

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# IN THE LABORATORY

## A Simple Automatic Pipette Washer

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More than a year ago I put together for my own use an automatic pipette washer made of such readily available materials and based on so simple a principle that I felt certain that the identical apparatus must have been constructed independently by many workers. However, during the past year the pipette washer has been seen by a considerable number of scientists, to all of whom it has appeared original. Seven of these washers have now been in use for some time in various laboratories in the Department of Animal and Plant Pathology and have given entirely satisfactory results.

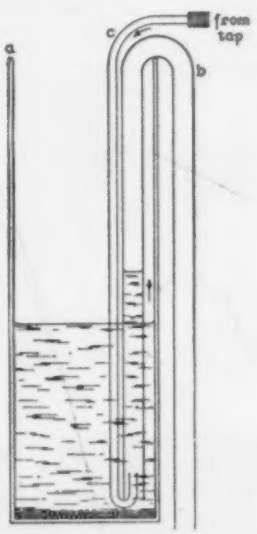


FIG. 1

The apparatus (Fig. 1) consists of a glass cylinder (a), 15 × 46 cm, equipped with a siphon (b) made out of tubing having a diameter of 2 cm or somewhat more. A calcium chloride tube connected by rubber to two straight pieces of tubing may be used. A piece of 8-mm tubing (c) is bent in the manner shown, and its upturned tip inserted into the bottom of the siphon. This tube (c) is connected with any convenient water faucet. The cylinder is placed near the edge of the drainboard of the sink so that water emptying through the siphon will run into the sink. It is convenient to have a layer of glass wool or glass beads in the cylinder to protect the tips of the pipettes which are placed in it. There should be a space of about 1 cm between the surface of the glass beads or glass wool and the open inner end of (b). When the water is turned on in (c), it will run first into (b) and then into the cylinder (a). As a result, a higher level of liquid is maintained in (b) than in (a). The difference between these two levels will determine the manner in which the pipette washer will operate. Best results are obtained if the flow of water into (c) is adjusted to a rate such that the level in (b) is 5 cm higher than in (a). With this adjustment, when (a) is nearly full, a siphon will be established in (b), and the cylinder will be emptied from the bottom in 10–15 sec, depending on the diameter of (b). The siphon then breaks, and the cylinder refills with water. In this manner the pipettes are submitted to repeated up-and-down flushing action which soon removes the trisodium phosphate or other cleaning fluid in which they have been immersed.

## Pollak's Trichrome Stain for Demonstrating Distemper Inclusion Bodies in Tissue Sections

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For the demonstration of distemper inclusion bodies in paraffin tissue sections, we have found Pollak's trichrome stain (*1*, *2*) to give good polychromatic differentiation between the inclusion body and the cellular elements. This method has been used with excellent results in staining sections taken from cases of mink, fox, and ferret distemper.

Any standard fixative may be employed. The slides are permanent; if properly stored, they are not likely to decolorize.

After the sections have been prepared and passed through the alcohols in the usual manner, they are stained in Weigert's iron-hematoxylin for 4–8 min, after which they are washed in running water for the same length of time. They are then stained in Pollak's trichrome for 7–15 min, the intensity of the stain being checked by the use of the microscope. Following rapid differentiation in 0.2% acetic acid and rapid dehydration in 95% and absolute alcohol, the sections are cleared in xylene and mounted.

If staining is satisfactory, inclusion bodies should stain crimson. Nuclei stain purple, and the cytoplasm of the bladder and trachea epithelial cells grayish-purple. Red blood cells stain orange; collagen stains blue-green and muscle red.

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# A New Method for Rearing *Drosophila*[1]

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The familiar half-pint bottle containing a culture medium has provided an extraordinarily simple and generally satisfactory means of rearing species of *Drosophila*. In fact, it is conceded that there would be no particular advantage in making a change if the maintenance of stock cultures were the sole or principal purpose in mind. However, in attempting to determine the important variables involved in population growth of *Drosophila*, the authors found it necessary to devise a new technique that would provide a better chance for the manipulation and control of these variables. The method to be described satisfied this need beyond expectation, and, in addition, offers many other advantages for research and teaching in the fields of genetics, physiology, ecology, toxicology, and general biology.

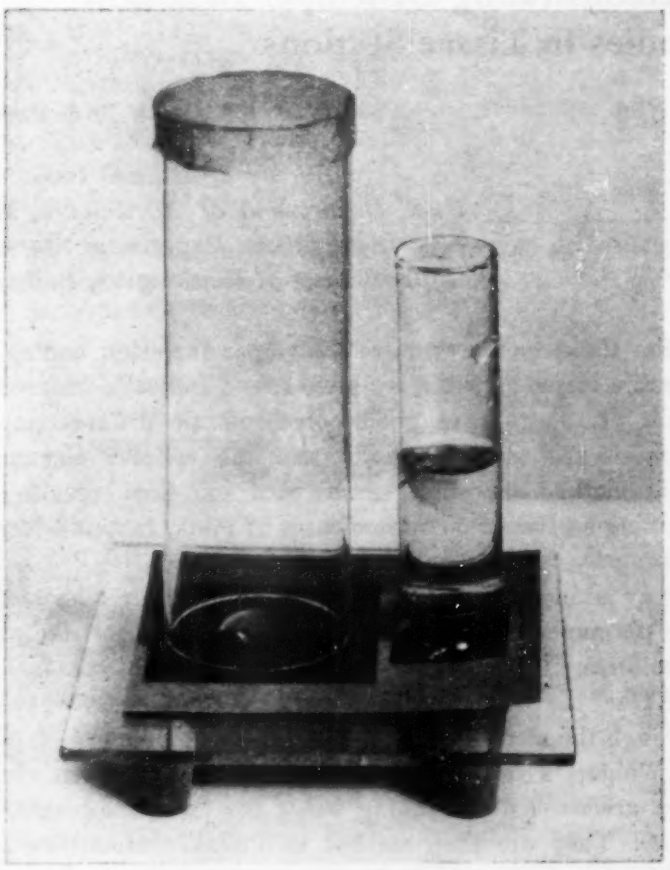

Fig. 1. Oviposition cage assembly.

The equipment used for the purpose of obtaining eggs is shown in Fig. 1. It consists of a base plate of glass, 4″ square, supported by cork-stopper legs held in place with Duco cement. A piece of seed-germinating blotting paper[2] is placed on the glass plate, two smaller pieces of the same paper (shown in the illustrations as black squares for contrast) being placed on the larger sheet. The oviposition cage consists of a glass cylinder, 4½″ high by 1½″ in diameter, covered with a silk bolting-cloth cap held in place with a double celluloid collar. The blotting paper assembly is kept moist, to a highly satisfactory degree, by water which is drawn from a 6-dram homeopathic vial used as a reservoir. A small notch filed in the lip of the vial permits an uninterrupted flow of water to replace that lost by evaporation.

The materials necessary for rearing larvae are the same as those described for oviposition with the exception of the rearing chamber illustrated in Fig. 2. This chamber is made from a glass cylinder 1½″ in both height and diameter. The bottom is sealed to prevent the escape of larvae by dipping the lower rim of the cylinder in melted beeswax; the cylinder is then pressed firmly on a separate square of blotting paper until the wax hardens.

Ordinary desk blotting paper can be substituted for germination blotting paper. The latter is better, however, particularly for use as the floor of the rearing chamber. Any loosely woven cloth that will prevent fly escape can be used instead of silk bolting cloth.

When eggs are desired, the first step in the process is to draw a circle, of the same diameter as that of the glass cylinder, on a square of blotting paper a little larger than the diameter of the cylinder. Then a line,

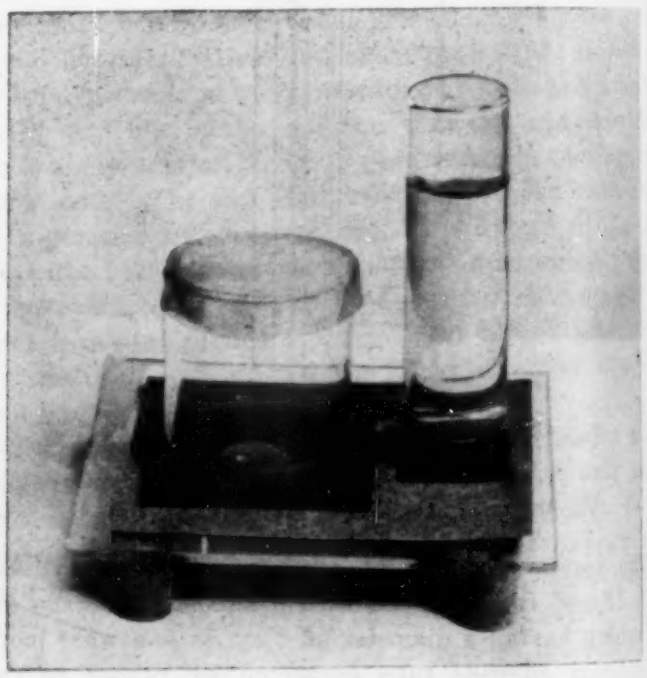

Fig. 2. Larval rearing chamber assembly.

marking off about one-third of the circle area, is scratched on the paper with a needle. Several other lines are scratched lightly across the surface of the remaining two-thirds of the circle drawn on the blotting paper. About 2 cc of a food consisting of equal parts by weight of cake yeast and honey is smeared on the surface of the paper in the smaller area marked out as shown in Fig. 3. Following this preparation of the feeding and oviposition surface, flies are introduced into the glass cylinder and the cylinder placed over the circle drawn on the blotter. They become adjusted very quickly and soon begin to oviposit, principally in the multiple scratches made on the free blotter surface. In the example illustrated in

[1] Paper No. 2367, Scientific Journal Series, Minnesota Agricultural Experiment Station.

[2] Manufactured by the Rochester Paper Company, Rochester, Michigan.

Fig. 3, 58 female flies of the species *D. melanogaster* were allowed to feed and oviposit for 3 hrs. In this short time they produced a total of 1,946 eggs, 94.6% of which were laid along the scratches, 3.1% on the food, and the remaining 2.3% on the free blotter surface between scratches. When eggs are laid in this way, they are easy to count and to remove.

The rearing of larvae is accomplished by the use of the set up shown in Fig. 2. Before eggs are placed in the rearing chamber a quantity of food consisting of equal parts of cake yeast and tap water is dropped on the blotter surface. This food is used instead of the yeast-honey mixture because it does not become sticky or ferment, and yet it produces large, vigorous flies. Again the homeopathic vial water reservoir serves to keep the entire system moist. When eggs are to be used, the oviposition chamber, with its blotter base, is inverted, and the top is tapped gently to force the flies downward. Then the floor of the chamber can be removed and replaced with a fresh oviposition surface. That portion of the blotting paper bearing eggs is cut off and placed on edge in the rearing chamber so that the lower margin

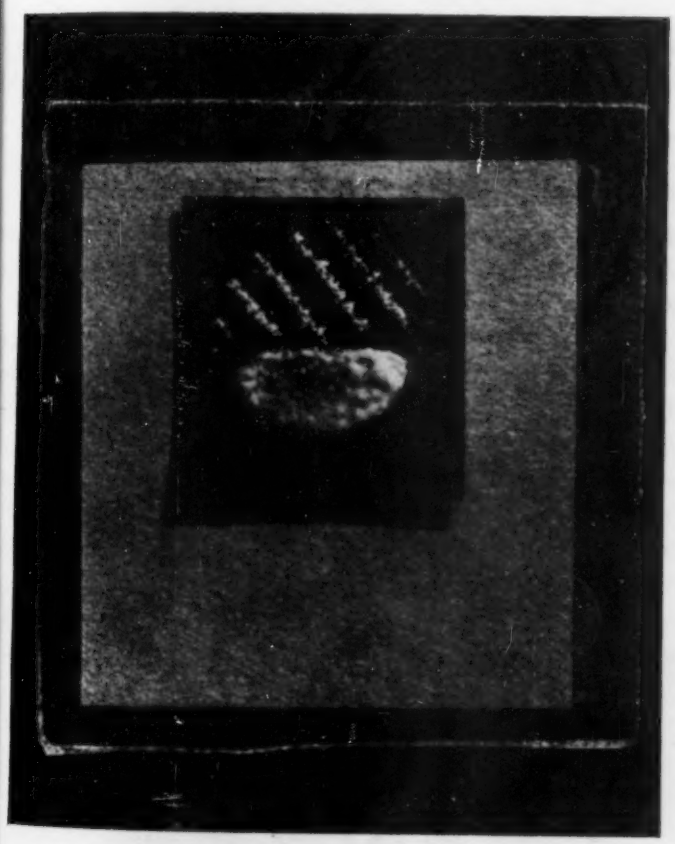

FIG. 3. Surface view of floor of oviposition cage showing food smears and rows of eggs deposited in scratches made on blotter surface.

makes a good contact with the moist floor of the chamber. This position of the egg-bearing piece of blotter is important to insure a thorough wetting and, further, to prevent the eggs from becoming submerged in the food mixture. Upon hatching, the larvae can move easily to the pool of food. Additional food should be added when necessary or, more specifically, at any time when the larvae start wandering around the chamber. If an adequate amount of food is supplied, there will be very little larval wandering and little tendency for them to bore through the blotting paper floor. Pupation is completed successfully without the necessity of providing a special surface. Frequently 1,000 or more flies of good size emerge in one of these small rearing chambers.

After emergence of the adults in the rearing chamber they can be removed by using either one of two methods. First, if the flies are to be anesthetized, the rearing chamber is inverted over an ether bottle with a mouth of about the same diameter. Second, when active flies are to be transferred, they are forced to the bottom by striking the rearing chamber sharply on the table top, after which the cap is removed and a glass oviposition cylinder set over the opening with a minimum of loss from escaped flies. Finally, the blotter prepared for feeding and oviposition can be slipped between the two chambers, and the cylinder moved back to the platform.

One of the important things upon which the success of the method depends is the maintenance of a supply of water to keep blotter surface and atmosphere above moist. The 6-dram vial used by the authors has supplied sufficient water to accomplish this purpose for about 24 hrs at ordinary room temperature and over a considerable range of relative humidity. The use of a small separate square of blotting paper under the water reservoir makes it possible to refill the reservoir without inverting the entire platform. The vial can be inverted easily by lifting it after a spatula has been slipped under the small square of paper. After the vial has been refilled and the paper square replaced, the vial can be inverted again without spilling water if a little care is exercised.

Among the most important advantages afforded by this new method is the fact that a ventilated system is provided which prevents the accumulation of gaseous products of metabolism; at the same time it is possible to maintain nearly constant moisture conditions in both the atmosphere and substrate. Within this open, yet humid, environment there is still another advantage over the standard milk bottle method. The food and substrate are easier to prepare and to maintain in a condition favorable for both oviposition and development. In addition, both the size of the system and the character of the food can be modified readily.[3]

As a result of these general advantages, with this technique, it is easier to measure such biological constants as fecundity, fertility, and survival and to isolate and place any growth stage under close examination or experimentation. It is also possible to produce large numbers of standard flies and to recover filial generations more easily, but it should be understood, that this method does not take the place of the usual *Drosophila* rearing method when only maintenance of stocks is desired.

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[3] Modifications of the standard method for the purpose of obtaining eggs have been developed by Alpatov (*1*), Harnly (*2*), and others. Although these methods greatly simplified the task of counting eggs, they still involve a tightly closed system and a variable substrate.

# Book Reviews

***Theory and applications of electricity and magnetism.*** Charles A. Culver. New York-London: McGraw-Hill, 1947. Pp. xii + 594. (Illustrated.) $5.00.

Prof. Culver's book, *Theory and applications of electricity and magnetism,* is a fine book for those who have just completed a first-year course in physics and are yet unprepared to plunge into a course in electricity and magnetism which would prepare them to cope with advanced electricity and magnetism courses such as those based on the material covered in Jean's *Electricity and magnetism,* Smythe's *Static and dynamic electricity,* or similar highly specialized courses in this field. Prof. Culver's book, in its attempt to be comprehensible to the undergraduate, who has had only the first year of physics, avoids the discussion of such basic topics as the solution of the Laplace Equation, the use of images, conformal transformations, and similar topics which must be presented to the senior student if he is to cope with the more advanced courses given in our first-rate universities.

The topics that are presented in this text are treated lucidly. Great care is taken to teach the student the use of proper units, and many examples are solved to illustrate methods of solution. The discussions of various measuring instruments are very effectively supplemented by diagrams and photographs, thus making the study of such instruments relatively easy. It is only in the case where a correct explanation of a concept, as, for example, polarization, leads to material for which the student is unprepared that Prof. Culver has to compromise and leave the reader aware that the explanation is neither rigorous nor convincing.

Few instructors who take their lectures seriously find the available textbooks to their liking. Almost everyone feels that he can write a better book or at least organize the material in a sequence that appears to him to be more logical, easier to present, or more convincing to the student. This is particularly so in the case of intermediate courses such as are usually offered in our larger universities to the senior physicists and engineers or to the first-year graduate students.

The difficulty of writing an intermediate textbook on electricity and magnetism, or mechanics, or heat, or atomic physics that would be universally acceptable is rooted in the fact that learning, itself, is a successive approximation process wherein each step is dissatisfying because it is only one of several steps that have to be taken before a satisfying understanding of the subject can be obtained. For this reason it becomes a matter of personal taste to a large extent as to how far one should carry a given topic before other topics are introduced. A clear example of this difficulty arises when one has to decide how much mathematics one should assume to be known. Electricity and magnetism, mechanics, vector analysis, and differential equations are all so closely tied in together that to single one out to be taught ahead of the others is almost entirely an arbitrary procedure. A physics student does not really obtain a satisfactory comprehension of vector analysis and differential equations unless illustrative problems in physics are used. The illustrative problems will make little sense unless he already knows the subject matter from which these are drawn. On the other hand, a good course in mechanics or electricity and magnetism cannot be given without vector analysis and differential equations.

Prof. Culver avoids this difficulty by limiting his subject matter to the level of advanced sophomore or junior year. It falls short as a text for advanced undergraduates.

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***Organic analytical reagents.*** (Vol. III.) Frank J. Welcher. New York: D. Van Nostrand, 1947. Pp. xi + 593. $8.00.

This third volume, like its predecessors, contains pertinent data on a large number of organic analytical reagents. The treatment is the same as in Volumes I and II (reviewed in this journal, July 18, 1947, p. 72; January 16, p. 74).

There are three parts in Volume III. The first deals with Heterocyclic Nitrogen Compounds, which includes chapters on, "Pyridine and Its Derivatives" (48 pp.), "Quinoline and Quinoline Derivatives" (15 pp.), "Dipyridyl and Related Compounds" (41 pp.), "Pyrazolone Derivatives" (12 pp.), and "Miscellaneous Heterocyclic Compounds" (37 pp.). Part II deals with the Oximes and presents chapters on "The Dioximes" (80 pp.), "Acyloin Oximes" (15 pp.), "Hydroxyoximes" (20 pp.), "Monoximes of Diketones" (5 pp.), "Isonitroso Compounds" (13 pp.), "Nitrosophenols" (49 pp.), "Miscellaneous Oximes" (16 pp.), "Cupferron and Neocupferron" (49 pp.), and "Nitroso Amines" (8 pp.). Part III, on Acidic Imino Compounds, contains chapters on "Rhodanine and Its Derivatives" (15 pp.), "Carbazides, Thiocarbazides, and Semicarbazides" (24 pp.), "Carbazones" (9 pp.), "Thiocarbazones" (90 pp.), and "Miscellaneous Imino Compounds" (25 pp.). The volume concludes with an index of names and synonyms of the organic reagents treated in the text, and one on their uses, listed in alphabetical order under the element or radical for which they are employed in analytical procedures.

Volume III, together with the others in the series, will make a ready reference set for all who have occasion to make chemical analyses.

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